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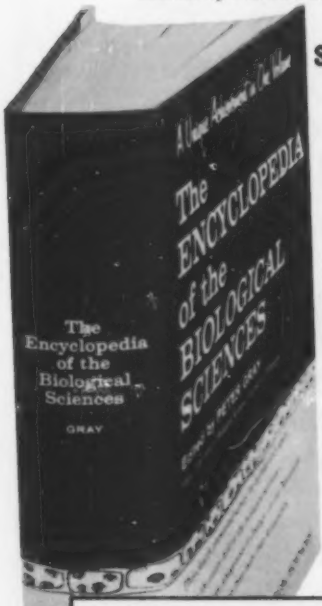

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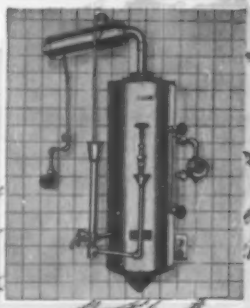

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THE QUARTERLY REVIEW
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SOVIET CELLULAR BIOPHYSICS 1950–1960

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ABSTRACT

The Soviet biophysical literature has been the subject of an extensive and critical review. The subject matter has been subdivided into sections on techniques, effect of ionizing radiation, effects of light radiation, photosynthesis, effects of ultrasonics, paramagnetic resonance, microorganisms, viruses, cell structure, proteins, nucleic acids, and muscle, nerve, vision and taste. A bibliography of 520 entries is included.

The work which has been reviewed covers the best and worst in Soviet science. In general those experiments and concepts in fields that are close to physics are excellent. Examples are the work in paramagnetic resonance, energy transfer mechanisms in photosynthesis systems, the properties of structural proteins such as procollagen, and the coding properties of DNA. The average paper concerned with more biological subject matter, such as microorganisms, viruses, and proteins, seems outdated in concept and lacking in controlled experiments or adequate data. Recent reviews (which contain mostly references to Western literature) indicate that Soviet scientists are well acquainted with work in Western countries and that the role of DNA as a carrier of genetic information is now fully appreciated.

INTRODUCTION

THERE is much evidence that scientific interest is growing in the field called biophysics. The Biophysical Society has come into existence, there are new journals devoted to biophysics and molecular biology, and there has been a big increase in the number of biophysics courses taught throughout the country. Part of the interest in biophysics has arisen because of the easy flow of ideas between scientists of the Western world, and the resulting cross-stimulation between scientific disciplines.

Many Soviet scientists are familiar with the work of the West, not only from reading translations of original articles, but even more from the many reviews of primarily Western work which appear in the U.S.S.R. Scientists in this country, however, are not well acquainted with Soviet work in the field of biophysics, despite the translation into English of such periodicals as *Biophysics*, *Biochemistry*, *Journal of Microbiology*, *Epidemiology*, and *Immunology*.

The purpose of this review is to acquaint scientists of the United States with Soviet cellular biophysics—the similarities and differences between their work and ours.

We have subdivided the subject of cellular biophysics into convenient, somewhat arbitrary categories. The categories are not mutually exclusive. They are:

1. Techniques
2. Effects of Ionizing Radiation on Cells and Cell Components
3. Effects of Light on Cells and Cell Components
4. Photosynthesis
5. Effects of Ultrasonics on Cells and Cell Components
6. Paramagnetic Resonance
7. Microorganisms

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8. Viruses
9. Cell Structure
10 Proteins
11. Nucleic Acids
12. Muscle, Nerve, Vision, Taste

1. TECHNIQUES

Light Microscopy

The subject of light microscopy and the absorption of light has been the subject of numerous reviews. The review by Agroskin (1958) is an excellent one. It is concerned with apparatus for cytospectrophotometry. The instruments described are Western types with the exception of one, which uses refracting optics, whereas the better modern instruments use reflection optics. Korolev and Agroskin (1957) describe some of the results of cytospectrophotometry in measurements on nerve cells in mouse brains. These results, which are similar to those of Caspersson in that they show a high concentration of nucleic acids in the nucleus, are reasonable but not surprising. A very good paper by Litvin (1958) describes modern electronic techniques (using twin T amplifiers and 2-beam photometers) which may be used to measure accurately the light transmission of samples.

A densitometer for use in scanning paper chromatograms has been described by Shipalov (1959). Adequate details are given, and the use of the instrument in measuring quantitatively the amounts of the adenosine phosphates is described, but no mention is made of the wavelength used. (Probably it was 2537 Å.) The samples necessary are about 50 micrograms, which yield transmissions on paper of about 30 per cent. There is no consideration of the possibility of eluting the samples into a small volume of solution, say 0.5 ml, in which case the transmission of a 1 cm thick solution would be 1 per cent; obviously in the latter case much smaller amounts of material could be used. While the technical details are excellent, just the addition of this point just mentioned would have transformed adequacy into excellence.

Methods for determining nucleic acid concentration by ultraviolet absorption are described by Spirin (1958). The essentials of technique are well known. They consist of extraction with 0.5 N perchloric acid at 100° for 20 min, and then measurement of the optical density of the supernatant solution at two selective wavelengths. The remarkable thing about this article is that, while there is reference to the Western volume *The Nucleic Acids*, edited by Chargaff and Davidson, there is no particular reference to the chapter on ultraviolet absorption.

The use of a modification of a phase microscope called an anoptral microscope has been described by Rukavtsov (1957, 1958). This type of microscope has an absorbing ring in the position usually occupied by the phase ring. As a result the background is dark and the image bright (in this respect it is equivalent to darkfield illumination). Unfortunately, in order to obtain good contrast, it is necessary to use a medium whose index of refraction is in the neighborhood of 1.4. Under this condition it is possible to get an idea of the existence of nuclear bodies in bacteria, but it seems to us that in all probability the microscope is being used slightly out of focus rather than in focus, and hence is really used as a phase microscope.

Fluorescent Techniques

A method of producing fluorescent dyes and coupling them to the γ-globulin fraction of serum has been described in excellent detail by Dashkevich and Mikhailov (1957). Fluorescent antibodies may be used to identify antigenic bacteria, but unfortunately the antibodies are not specific enough to be used for quantitative identification. The use of fluorescent techniques to detect lipids (by use of caffeine benzpyrene) and to detect nucleic acids (by using acridine orange) has been described by Breivis (1957), although he gives no good experimental data on the specificity of such dyes.

The use of fluorescent techniques to determine numbers and properties of bacteria has been described by Vasilov, Rassudov, and Radionova (1957). The concentration of bacteria they used was around 2×10^9/ml, which seems to be well above a useful concentration range. Derkacheva, Zhevandrov, and Khan-Magometova (1959) have described a luminescence method, in which only adsorbed dyes luminesce, to determine bacterial concentrations. The method is only good for concentrations greater than 3×10^7 bacteria/ml, a value which is well within the range of the simpler, conventional turbidimetric techniques. Other fluorescent techniques applied to microorganisms are discussed in Section 7.

Sysoev and Martsinkevich (1957) have written on the determination of stimulators of growth in

yeast by nephelometry. Instead of measuring the growth rate of different extracts as compared to yeast grown in minimal media, they kept checking control cultures until they reached an optical density of 0.1 and then measured the turbidity of cells in the extract under test. They did this with ten control tubes which they fixed and tested two at a time, instead of having apparatus which tests samples without destroying them! The following is an example of the type of description of experimental technique in this paper:

> Sterile test tubes (with cotton plugs) were filled under sterile conditions with one milliliter of the experimental extract in corresponding dilution (usually two dilutions were tested, one from the optimal zone, and another more concentrated).

The paper contains no mention of why various growth responses arise.

X-rays

Some applications of X-ray techniques are described in the section on proteins. In addition to these structural studies there are two groups of workers working on X-ray shadow microscopes. They are those of Bagdyk'iants (1956, 1959a, b) and Rovinskiy (1956, 1959). Andreyeva and Iveronova (1957) have given theoretical calculations for the structure factors of high polymers; the equipment used for dosimetry measurements in X-irradiation has been described by Khonazuk (1957). All these papers seem technically competent.

Electron Microscopy

There are many descriptions of electron microscopes of recent design. The best of these are those by Chentsov, Vertsner, and Bogdanovskiy (1959), Vertsner et al. (1959), as well as that by Popov (1959), who describes the operation of an electron microscope working at accelerating voltage of 400 kv.

Equipment for shadowing samples for electron microscopy is described by Draganov and Skalinskiy (1957). The equipment is best described as a do-it-yourself affair of good quality. As compared to commercial units available in this country it lacks flexibility.

Gileyev (1957a, b; 1958) has described what seems to be an excellent embedding technique, namely, the use of gelatin for preparation of thin sections for electron microscopy. The technique seems to show details which are not seen with the usual plastic embedding. He has applied this technique to the study of the regeneration of striated muscle. The application of electron diffraction techniques to the study of the crystal structures of chlorophyll *a* and *b* and gramicidin has been described by Belavtseva (1957, 1959). The technical problem of the deterioration of the crystal under bombardment by the electron beam seems to have been solved by the use of very low current densities.

Some diverse applications of electron microscopy other than those described in the sections on Viruses and Microorganisms are those of Bogoiavlenskiy (1958) on the determination of the structure of the cuticle of nematodes (he found two more layers in the cuticle than the six found by light microscopy) and Morozova and Belavtseva (1958), who have examined chloroplasts after osmium fixation. The chloroplasts were not sectioned and so no structure could be expected to be seen, and none was found.

Miscellaneous

Miscellaneous techniques are, for example, those concerned with the quantitative determination of antibody-antigen reactions by observation of the precipitin reaction in gels. This has been the subject of two good reviews, one by Gendon (1958), which contains no references to Russian work, and another by Lebedev and Tsilinskiy (1958), in which only 15 of the 107 references referred to Russian work. In neither of these articles is there reference to the newest technique of combined gel diffusion and electrophoresis. Apparatus for protein electrophoresis on the micro level, that is, using one-half a milliliter of serum, has been described in detail. The ideas, remarks, and description of this apparatus are excellent (Troitskiy and Kobozev, 1958).

Some recent radioautographic techniques have been described by Mamul', Orlova, Shuvatova, and Kuzin (1958). They describe a method whereby artifacts due to the diffusion of small molecules are eliminated by use of frozen sections and the exposure of films at liquid nitrogen temperatures. (At these temperatures exposure times are 25 times longer than at room temperature.)

The subject of radiotopography, i.e., the visualization of parts of the body by the location of radioactive isotopes within it, has been described by Shmelev (1959). This review paper also has no reference to Russian work. Much of the de-

scription is too general to be of any use. For example,

> The choice of tracer is decided by the half life, by the decay scheme (the positron branch should be strong and other γ-rays should be absent), and by the tracer's biological behavior.

Unfortunately, the review contains no good description of how to make the decision as to the relative importance of these conditions, or what items to consider in deciding the relative importance.

Zviagintsev (1959) has described studies relating to the adsorption of microorganisms to glass surfaces. The big variable is not the type of glass but the species of microorganism and the cleanliness of the glass, that is, whether it is free of organic matter (in which case the adsorption rate is small).

2. EFFECTS OF IONIZING RADIATION ON CELLS AND CELL COMPONENTS

Attempts have been made to use ionizing radiation for two useful purposes besides the well-known one of producing mutations: (1) to eliminate the infective property of microorganisms without destroying their ability to make antibodies (Leshkovich, 1958; Pshenichnov, 1959; Tumanian, Duplishcheva, and Sedova, 1957—a careful job); and (2) to determine the molecular weight without the necessity of purification (Meduski and Volkova, 1957; Budnitskaya, Borisova and Pasynskiy, 1956; Volkova, Tongur, Chunaeva, and Pasynskiy, 1957).

Effect of Ionizing Radiation on Macromolecules

The Institute of Biological Physics produces much of the work on this subject. Irradiations of whole organisms are followed by investigation of individual cell components, or macromolecules are isolated from cells before irradiation. Kuzin and his collaborators have made several interesting correlations between macromolecular components and cell sensitivity, such as that old chloroplasts with low nucleoprotein content are less sensitive than young ones, and that the number of free phosphate groups in cellular nucleoproteins, as measured by isoelectric point, is associated with the radiosensitivity of different types of cells within one organism (Kuzin and Shabadash, 1958). Kuzin and Budilova (1958) have followed the decrease in P/O ratio after whole-body irradiation in rats. Normal DNA added to a mitochondrial suspension from an irradiated animal will restore the P/O ratio, but irradiated DNA loses the ability to do so. There are many macromolecular studies after whole-body irradiation, such as that of Epshtein and Zabozlaeva (1956), of the Department of Biochemistry, Stalinabad State Medical Institute, on serum albumin changes in dogs produced by incorporated Fe^{59}. Larionov, Manoilov, Ryskina and Sorokina (1956) found that protein splits off from nucleic acid in X-irradiated rat sarcoma tissue, as shown by spectral changes, and subsequently the DNA depolymerizes and some of it migrates out of the cell. Many other authors (such as Toropova and Ermolaeva, 1958; and Libinzon, 1959) report DNA decreases in tissues of irradiated animals. However, Brodskiy and Suetina (1958) believe that cell damage is not *caused* by reduction in nucleic acid, but rather by alteration in nucleic acid metabolism. Their conclusions are based on ultraviolet cytophotometry of bone marrow. The peroxide content of lipids extracted after whole-body irradiation is reported to increase at the same rate as in irradiation in vivo (Tarusov, 1958).

Among the studies of radiation effects on isolated proteins and nucleic acids and their components many are concerned with the change in the absorption spectrum (Rysina and Libinzon, 1958; Pavlovskaya and Pasynskiy, 1955). The former workers, on the basis of good data, report pyrimidines to be more sensitive to gamma rays than purines are, like the result found earlier in the Western world. Zaides and Sinitskaya (1951), Zaides and Stoianova (1956), as well as Karpov (1957) and Karpov and Zverev (1955), have investigated the change in the diffraction pattern of various macromolecules on electron bombardment. Tongur, Golubeva, Diskina, Spitkovskiy, and Filippova (1957) have found a change in viscosity and ultraviolet absorption of DNA-proteins with as little as 1 kr, whereas Budilova and Kuzin (1957) report that 20 kr is necessary for this effect. Volkova and Pasynskiy (1955) agree that low doses will change viscosity. Kuzin and Budilova (1956) claim that dissolved DNA is sensitive to 5 kr if traces of trivalent iron are present. Pasynskiy (1955) mentions that 50–500 r has an effect.

Oppel (1959), of the Military Medical Academy, Leningrad, has found detachment of low molecular weight peptides from hemoglobin irradiated with gamma rays in solution, although

the electrophoretic mobility and pepsin digestion was unchanged. Khenokh (1950) and Khenokh and Lapinskaya (1955, 1956) have likewise found protein splitting by ionizing radiation. Gamma-ray effects on silk were investigated by Arifov and collaborators (1958).

The work of Eidus and his collaborators and of Kuzin and Plyshevskaya (1956) on myosin and its protection by complexing with actin is an unusual study in that they found that when the complex was formed *after* irradiation and then the myosin separated out, the adenosine triphosphatase (ATPase) activity of the irradiated myosin was higher than if no complex had been formed. Eidus, Kondakova, and Otarova (1958) claim that myosin irradiated in a vacuum is subject to enhancement of irradiation effects by oxygen let in *after* the irradiation is complete, suggesting a long-lived excited state which can combine with oxygen. This excited state is believed by Eidus and Ganassi (1959) to account for the kinetics of thermal inactivation of irradiated myosin solutions. A similar argument is used to explain photodynamic action (Eidus and Kondakova, 1959). They have apparently confused a well-defined physical concept, the excited state, with an undetermined unstable condition of irradiated protein.

Several studies of inactivation of serological properties of proteins by Zil'ber and Artamonova (1959) and by Remennikova (1956) are of interest. Remennikova found that beta and gamma rays from radon had no effect on either the precipitin or the complement fixation reaction of native horse serum. Alpha particle irradiation, however, had an effect—larger on the globulins than on the albumins. X-rays of 330 kr had no effect on the combining power of immune serum from rabbits, but antibodies lost their serological properties with beta and gamma radon doses of 15 mc days, O antibodies being more sensitive than H-antibodies. Kaulen and Chakhava (1958) of the Gamaleya Institute have made a good study of serological properties of irradiated diphtheria antisera. Their conclusion is that gamma rays are a poor way of sterilizing antitoxins.

Burlakova, Dzantiev, Sergiev, and Emanuel' (1957) report oxidation of irradiated fish oil and conclude that ionizing radiation in general may involve such effects. Zhuravlev (1957) also worked on physiochemical changes in irradiated fat (from ox liver).

Effects of Ionizing Radiation on Cells

The genetic effects of irradiation are widely studied. The chromosome aberration work is characterized by lack of breakdown into types of aberration in scoring damage. Shapiro (1958, 1959), for example, has irradiated whole mice or rats. He reports that there is a decrease in division rate and that the proportion of cells that contain aberrations remains constant for a long period. However, regenerated liver contained few aberrations. Nuzhdin and Domareva (1959) have done the same type of experiment, with the equally trivial result that recovery takes longer when the dose is higher. Tiniakov and Arsen'eva (1958a, b) have made cytological investigations of irradiated monkeys for two years after irradiation with 150 or 400 r. Even after such a long interval there are still 5–6 per cent of cells with anaphase aberrations. Kerkis, Ronichevskaya, and Rukavishnikov (1959), of the Institute of Cytogenetics, Siberia, found aberrations in guinea pigs irradiated with only 4 r. They found monkeys less sensitive. Dubinin (1957a, b) concludes from this type of study that one should beware of a foreign "maximum permissible dose." Neifakh (1956) and Neifakh and Rott (1958) have examined the effects of X-rays on loach germ cells in terms of the damage expressed during various multicellular stages (blastula and gastrula). They interpret the results as either a lag in appearance of defects, or as indicating that the pre-irradiation activity of the nucleus has guaranteed development only up to a certain point. Ivanitskaya (1959a), of the Severtsov Institute of Animal Morphology, has irradiated explanted spleen and then examined the effects on subsequent culturing. She finds that different types of cells have different sensitivities, with a possible correlation between resistance and nucleus-to-cytoplasm ratio.

Breslavets (1958) presents some data indicating that the nucleus of resting cells is less sensitive, but that the mitochondria and plastids are *more* sensitive to radiation in the resting cell.

There are numerous discussions of the theory of ionizing radiation effects. Tarusov (1954, 1957a, b) rejects the target theory because there is indirect action. Pasynskiy (1957) suggests that there is "biological amplification of radiation damage," since damage to only a few molecules can cause profound changes in the cell, and that possibly a monomolecular or bimolecular interface is involved in this amplification. Timofeyev-

Resovskiy (1956) of the Biophysics Laboratory, Sverdlovsk, likewise considers that radiation damage occurs only in highly localized areas. Privalov (1958) points out that one can explain the fact that damage in radiation effects is concentrated at surfaces on the basis of the ordered structure of water. Effects may be transferred across aqueous regions by proton transfer, and protons can pile up at surfaces. Kuzin (1957) considers that the main effect of ionizing radiation in biological systems is on DNA.

Korogodin (1957a, b, and c; 1958a), of the Department of Biophysics, Moscow State University, has done a meticulous piece of work on the kinetics of post-irradiation changes in yeast. He gives data on the division rate after various doses, and he determined when irradiated cells lose the power to divide. Different results were obtained for haploid and diploid strains. His fundamental conclusions, such as that some of the cell deaths are possibly non-genetic, may not be correct; but he presents a great many careful data. Shekhtman, Plokhoi, and Filippova (1958) have done an interesting experiment involving the determination of the shape of the *Escherichia coli* survival curve with X-rays and with polonium alpha particle irradiation. In 2 per cent glucose solution these cells, which are then much larger than normal, exhibit a multi-target curve, whereas they are usually inactivated exponentially. Motuzova (1957) also finds exponential survival in water, but not in broth unless it is dilute and the culture is young.

Much work has been done on the effect of radiation on various properties of cells besides their ability to divide and mutate. For example, Rubin and Mikheyeva (1958) have found that the phospholipids of mitochondria of irradiated potato tuber cells increase by a factor of about two. Inositol has been found to increase in irradiated yeast (Seliverstova, 1957, 1958); and these organisms, when irradiated, release pantothenic acid into the medium. Sokurova (1958a) claims that unstable yeast mutants which produce abnormal amounts of ergosterol are induced by X-rays, but there is no genetic evidence to back up the claim. Gal'tsova, Meisel', and Seliverstova (1954) obtained the same type of result with yeast. Gal'tsova (1955) also found an increase in the number of free sulfhydryl groups in irradiated yeast, which was maintained for 100 divisions before starting back to normal! Gal'tsova and Meisel' (1958) measured a decrease in amination, and transamination, and an increase in deamination after yeast cells were X-rayed. Seliverstova (1959) found more para-aminobenzoic acid production in X-rayed yeast, a result which is interpreted as a disturbance in nucleic acid metabolism. Aerobic irradiated yeast cells show a decrease in cytochrome oxidase (Meisel', Gal'tsova, Medvedeva, Pomoshchnikova, Seliverstova, and Shalnova, 1958). Results on the change in electrical resistance (Trincher, 1959, Institute of Biological Physics, Moscow State University; Kriger and Elkhovskaya, 1957, 1958) and permeability (Kuzin and Ivanitskaya, 1957; Ivanitskaya, 1959b) of erythrocytes as a function of radiation are interpreted as surface macromolecular changes.

Similar permeability studies are reported on the cells of higher plants. Vasil'ev, Ch'in Su'-yün, and Rybalka (1958a) measured the amount of material given off by wheat seedlings and roots, a process which does not begin until more than a week after irradiation with 30 kr or less. This paper, like many in the Soviet biological literature, lacks an appreciation of order of magnitude. The results are given to five significant figures, and the error is given with four! An increase in oxidation, found in irradiated tobacco leaves, is discussed by Virovets and Pasynskiy (1959a, b), who give the interesting interpretation that disturbing the leaf structure by radiation makes possible more contact of enzymes and substrates in oxidation, analogous to the increase in oxidation when leaves are crushed. Kachalova (1957) makes the same suggestion. Studies on the effect of irradiation on chloroplast size (Vlasiuk and Bidziliya, 1958) and on chlorophyll and hemicellulose contents (Vasil'ev, Ch'in Su'-yün, and Rybalka, 1958b; Bidziliya, 1958) and photosynthetic activity (Kuzin, Sun Ch'ih, and Sayenko, 1958) are mostly listings of the effects, with little or no attempt to investigate the mechanisms involved. Protein synthesis in rye shoots (Sisakiyan and Kalacheva, 1957) is stated to be depressed by 10 kr, but *increased* by 5 kr.

The toxic effects of irradiated media on unirradiated cells are reported in a number of papers (Ivanitskaya, 1959c; Meisel', Gal'tsova, Medvedeva, Pomoshchnikova, Seliverstova, and Shalnova, 1958; Kuzin, Kruikova, Sayenko, and Yazykova, 1959); but none of these investigators seems to have any idea of what the toxic substances are. Zinov'eva (1958) claims an effect of an irradiated simple salt medium on paramecia if the animals

are first sensitized to it. She reports that the toxic effect of the medium on the animals remains for a number of hours. Graevskiy and Zinov'eva (1958) believe that infusoria die because of indirect effects alone, since peptone added to the medium protects the animals.

Radiation Modifiers

There is little work on the modification of radiation effects on macromolecules. One example is the work of Pavlovskaya and Pasynskiy (1957), which is an unsophisticated study of the effect of air (oxygen) on the radiation sensitivity of protein solutions, as judged by changes in absorption spectra. The authors do not make a proper correction for the turbidity of their solutions.

There is a great deal of literature on the modification of cellular damage by various agents, much of it characterized by a maximum of theorizing and a minimum of data. Important biological details are often ignored. For example, Graevskiy (1952) and Graevskiy and Ochinskaya (1953) irradiated one billion *E. coli* cells per ml, with no mention of the fact that the pH is altered at this high concentration of cells. Their data are badly scattered, and many of their conclusions, such as that the action of radiation is *not* an effect on specific macromolecules in the cell, seem to have little or no relation to their data.

Some studies on radiation modifiers, those of Litvinova (1959a, b), at the Institute of Animal Morphology, for example, while not original, are careful. Litvinova found that post-irradiation temperature has less effect on the division rate of irradiated paramecia than on that of controls. Likewise Luchnik (1958) and Luchnik and Tsarapkin (1959) found a decrease in chromosome aberrations when yeast extract and cysteine were applied after irradiation of pea seeds with cobalt-60.

The oxygen effect on the action of ionizing radiation has received considerable attention. A book on this subject by Shchepot'eva, Ardashnikov, Lurye, and Rakhmanova (1959) contains many more Western than Russian references; and the authors seem to know about the important Western literature. There is considerable confusion (and argument) in the literature as to whether oxygen has an effect only *during* irradiation (Eidus and Ganassi, 1958) or whether it is particularly effective *after* irradiation (Shapiro, Bocharova, and Belitsina, 1959). Kalantarov's (1958, 1959) and Tambiyev and Kalantarov's (1958) studies on the oxygen effect in yeast are much less trivial than most oxygen effect studies in that they distinguish between no oxygen effect on cells which die during irradiation and considerable oxygen effect on cells which die 16 hours later. They find a maximum effectiveness of oxygen pressure at around 300 mm Hg, above which the effect drops. This is contrary to oxygen pressure sensitivity curves, obtained in Western countries, which show an increase in sensitivity with increasing pressure, and a final leveling off of the effect at higher oxygen pressures.

Studies on the effect of temperature on radiation damage, such as in yeast (Meisel' and Medvedeva, 1954) and paramecia (Smirnova, 1960) are very simple. In general, cells with higher ploidy are seen to have greater resistance (Tul'tseva and Astaurov, 1958; Mansurova, Sakharov, and Khvostova, 1958; Dubinin, 1957c), a result well-known for a long time. The division stage of cells is shown to be an important factor in sensitivity to chromosome breaks or death, as in the *Tradescantia paludosa* microspore (Delaunay, 1958c) and the loach embryo (Belyaeva and Pokrovskaya, 1959). Vakhrameyeva and Neifakh (1959), of the Severtsov Institute, have investigated the X-ray and temperature sensitivity of loach eggs as a function of division stage, and they report the interesting result that the cycles of sensitivity to the two types of inactivation do not coincide. There is about a half of a mitosis of lag between them. The nature of the post-irradiation medium is also an important condition. It was shown by Korogodin (1958b) that the recovery of yeast cells after irradiation is much higher when the cells are incubated on non-nutrient agar. High cell concentrations (Korogodin, Tarusov, and Tambiyev, 1959) result in greater damage. Protection by fats (in *Endomyces magnusii*, reported by Alekseyeva, 1958), by hyposulfite (in *Allium*, Dubinin, Sidorov, and Sokolov, 1959), or by substances which block cell respiration (Meisel', Pomoshchnikova, and Sokolova, 1957) is sometimes correlated with the oxygen effect. Delaunay (1958a, b), who shows a good knowledge of Western work on chromosome breakage, has worked on the additivity of the effects of ethylenediamine-tetraacetic acid and radiation on *Tradescantia* microspores, and has good experimental data with sensible interpretations of them. Tumanian (1957) found that concentrations of phosphorus-32 and streptomycin that have no

effect on *E. coli* and *Staphylococcus* when applied separately can be lethal when combined.

Dose rate studies have been done by Shekhtman, Radzievskiy, Zotikov, and Glazunov (1958) and by Luchnik (1956). Some workers (Ardashnikov and Chetverikov, 1958) calculate the dosimetry involved with a three-dimensional target.

The energy given off by naturally occurring radioactive substances is not considered great enough to be important in organic synthesis (Kuznetzov, 1956).

There is a lack of interest in variation in sensitivity with linear energy transfer. Sokurova's (1958b) paper is the only one we have found on this subject. She merely compares the effectiveness of alpha and beta rays in depressing *Azotobacter* growth (alpha particles are more effective). Many investigators are inclined to rely on a single dose, or a small range of doses, as a basis for conclusions on the general effects of radiation. Kalabukhov (1957) warns that to evaluate even the effects of antibiotics one needs to know the shape of the dose-response curve and not just the effect of one dose.

Irritation with the practitioners of the art is expressed thus by Pushkarskiy (1957): "The enthusiasm for influencing plants by irradiation has not justified itself; there has developed a mass of misshapen forms incapable of surviving."

3. EFFECTS OF LIGHT ON CELLS AND CELL COMPONENTS

The amount of work done in this field is small compared to that done in the field of ionizing radiation. There are two good centers of this type of research in the socialist countries outside the U.S.S.R. One is in Yugoslavia under Kanazir, and the other in Poland under Shugar. The work of these groups will not be reviewed here.

Microorganisms

The work on the action of light on microorganisms suffers because of the poor work done in microbiology. Most of the work is concerned, for example, with the immediate practical matter of producing more and better mutants to obtain antibiotics. This work, primarily under the direction of Alikhanian, has been concerned with the combined effects of such drugs as ethylenimine, of ultraviolet light, and of X-rays on actinomycetes, so as to find methods to produce strains which make more active streptomycin and terramycin. There is no quantitative work on the fundamental causes of cell death or mutation under the action of light; but the workers have shown that the combined effects of drugs and X-rays are often greater than the sum of the individual effects, and moreover that the photoreactivation of inactivated bacteria yields proportionately more mutants than if there had been no reactivation. Sokuruva and Meisel' (1957) have shown that the sum of the effects of X-rays plus ultraviolet is greater than the sum of the individual actions. This is in disagreement with work reported on yeast by Uretz and Sarachek in the United States. Lozina and Lozinskiy (reported by Vorobiev and Zhirmunskiy, 1958) allege that infusoria develop resistance to ultraviolet light following previous small doses. This effect might result from the survival of more resistant animals or from a delay in division induced by the first ultraviolet dose.

Photoreactivation

A more or less typical paper (coming from the Institute of Biochemistry and Biophysics in Poland) is that by Grundland, Krzywicka, and Chojnacki (1958) on the mechanism of photoreactivation following ultraviolet irradiation. It is worthwhile considering this paper in some detail, since similar criticisms hold for many of the other papers. The authors hypothesize that ultraviolet light depolymerizes nucleic acids and that visible light leads to their reconstitution. (Shugar would probably say that ultraviolet light may polymerize nucleic acid rather than depolymerize it.) Therefore, the authors argue that after ultraviolet irradiation there will be many fragments of nucleic acid polymers and these fragments will change the surface charge of bacteria. They then proceed to investigate the electrophoretic mobility of irradiated cells. The experimental procedure, which is given in insufficient detail, is to grow cells in a nutrient medium, resuspend them in acetate buffer at 4×10^9 cells/ml, and then irradiate them with light of wavelength 2537 Å. (But at the cell concentration and thickness used, which was of the order of millimeters, the amount of incident light that would get into the irradiated sample is almost impossible to calculate and even though the authors give the dose incident on the surface, this is a meaningless number.) The cells are kept at this high concentration, and the optical density and the surface charge are measured. After 24 hours the optical density rises by about 50 per cent, and the surface

charge falls by about 30 per cent. It is concluded from these, and slightly more data, that "the immediate effect of ultraviolet irradiation is to cause depolymerization of DNA, the fragments of which are initially attached to cells. Photoreactivation is possible in this phase." This conclusion, drawn from evidence more or less all of which has been cited above, is preposterous.

Photodynamic Action

In the experiments of Eidus and Kondakova (1958, 1959) and of Engel'hardt (1950) on the mechanisms of photodynamic action in the system of methylene blue on myosin, there is also a tendency to draw extravagant conclusions. In some of these cases there is no evidence that the protein preparations were pure. The experimental data indicated that if myosin and methylene blue were irradiated in oxygen-free solution by visible light and assayed for ATPase activity, the measured activity was higher in O_2-free solution than if assayed in a solution containing O_2. The loss of activity in oxygen-free media was never more than 25 per cent, while in oxygenated media it reached 100 per cent; but these results were taken as evidence for a long-life state. These investigators further concluded that myosin is made up of $4n$ units, only one of which shows a long-life inactivation. There was no mention of the many secondary reactions which could go on in the inactivation of enzymes by light, and no mention of the possibility that the total amount of dye in the system is only sufficient to inactivate one quarter of the myosin molecules if oxygen were not present. The conclusions may be correct, but the data and the necessary logical analysis were not given.

Fluorescence and Energy Transfer
(see also Photosynthesis)

The experiments on the fluorescence of amino acids, proteins, and amino acid crystals are equivalent in many respects to those carried on in the West. The apparatus and the techniques are usually excellent. This finding is typical for experiments which are closer to the physical rather than to the biological side of Russian science. The results obtained by Barskiy and Brumberg (1958) on the fluorescence of amino acid crystals agree with those obtained in the Western countries and with those of Konev (1957a, 1959) on the fluorescence of proteins. The action spectra for production of fluorescence are interesting, but the necessary information about many experimental aspects is missing. Barskiy reported that hydrolysates and amino acid mixtures have action spectra which correspond to the absorption spectra of aromatic amino acids, but that proteins have action spectra with a second peak at 245 mμ instead of only a single peak at 280 mμ. (There was no good evidence for a pure system.) Nuclear proteins, on the other hand, show action spectra which look more like protein absorption spectra, and denatured proteins show more fluorescence than native ones. The technique is a good one and has been much more extensively exploited in work on photosynthetic pigments.

The best review of the various mechanisms of energy transfer as applied in particular to biological systems is that of Vladimirov and Konev (1957). There is no equivalent review in the Western literature.

Vladimirov (1957a, 1959) has investigated experimentally the energy transfer from phenylalanine to tryptophan by sensitized fluorescence and has shown that the action spectrum for the fluorescence of yeast is like the absorption spectrum of the aromatic amino acids, even though a large fraction of the incident light is absorbed by the nucleic acids of yeast cells.

Ril' (1955a, b) concluded, somewhat belatedly compared to Western workers, that heat cannot account for the photo-dissociation of carbon monoxide from the protein myoglobin, but that some energy transfer mechanism must be involved. He investigated the photoconductivity of gelatin films, but the results are probably meaningless, since traces of salt would influence the results by orders of magnitude more than any expected conductivity of pure proteins.

Effects on Proteins

Work on the effects of light on proteins covers a wide range of values. An excellent discovery is that of Pasynskiy (1958), who found a relationship between thermal and ultraviolet inactivation constants of proteins. Obviously, the discovery required a good knowledge of the Western literature and of the various theories concerning ultraviolet inactivation. His argument to explain the proportionality between $\Delta H^{\ddagger}$ and the quantum yield is ingenious, even though the present reviewers do not believe it. At a similar level of competence is the work of Kolomiychenko (1956, 1957, 1958a, b) on some of the enzymatic and physicochemical properties of proteins after irradiation. At the other extreme, there is work which is of very little

use because of the lack of quantitative data, such as the measurement of the photo-oxidation of sulfhydryl groups (Kofman, 1957), with no intensity measurement.

The work of Boldyreva (1955), on the effects of various spectral regions on the enzyme D-amino acid oxidase, is incomprehensible as far as its results are concerned. Boldyreva found no effect of short wavelengths on the enzyme, but a big effect for long wavelengths. Again, not enough data are given to evaluate the result, but it might very well be that, because of the preparation used, the shorter wavelengths never got into the solution and the longer wavelengths, even though they are relatively inefficient, at least were able to penetrate to a sufficient depth to inactivate an appreciable fraction of the enzyme. Alternatively, the effect may be one of photodynamic action on the prosthetic group.

Volkova and Pasynskiy (1955) irradiated lipoproteins and nucleoproteins with ultraviolet light of doses of 10^3 ergs/cm^2 and found depolymerization of nucleoproteins and polymerization of nucleic acids. (Note that this effect of ultraviolet on nucleic acids is the opposite of that assumed by Grundland et al., mentioned above in the section on photoreactivation). Lipoproteins were much more stable. Obviously, nucleic acids which absorb so much more light than proteins, will be affected first. The conclusion which was drawn from these results, namely, that nuclear proteins are built up from block segments and are bound to one another by nucleic acid, is farfetched.

Vaccines

Some work has been done on the production of virus vaccines by ultraviolet light. A study by Levkovich, Gol'dfel'd, and Rzhakhova (1957) of the Ivanovsky Institute of Virology, Moscow, indicates that the authors knew very little about ultraviolet light or even how to assay properly for the survival of virus infectivity. They irradiated tissue extracts containing encephalitis virus at an extract concentration of between 1 and 10 per cent. As in the work of Grundland mentioned above, the absorption of the samples was so high that a measurement of the incident intensity leaves one unable to calculate the average intensity through the sample, and the dose of radiation was completely non-uniform. Their irradiation times were between 5 and 15 minutes for 2537 Å and wavelengths above 2900 Å. The long wavelength did not affect the antigenicity; the shorter wavelengths were only effective for times greater than 15 minutes. The infectivity tests were done on five mice for each dose, unfortunately all at the same dilution. For zero dose, five out of five mice died, and for the 15-minute dose none of the five mice died; but how to connect these two points is something that the authors do not seem to have thought about.

Mitogenetic Radiation

Mitogenetic radiation, or radiation which cells are supposed to emit while undergoing mitosis, was thoroughly discredited about 25 years ago by Hollaender. It has not died in the Soviet Union. This radiation, which is supposed to be in the short ultraviolet, ~2000 Å, can only be detected by the proper type of biological detector. We now behold the analysis of the fine structure in mitogenetic radiation. (The fine structure seems much more pronounced than that observed in absorption spectra). Slavina (1959a, b) claims that an analysis of the $-NH_2$ and $-OH$ frequencies reveals that the framework of cancer cells is different from that of normal cells. No experimental details are given in this work, and the subject makes *no* sense by our standards. Eremeyev (1958) has investigated the importance of these "rays" in the synthesis of peptides in mouse liver, and Peredel'skaya (1954) has related mitogenetic radiation to chlorophyll and carotenes.

4. PHOTOSYNTHESIS

Techniques

Equipment for measuring intensities in the spectral range associated with photosynthetic processes has been described by Andreitsev, Il'ianok, Osetrov, and Sokolov (1957), and the elaborate equipment necessary for the recording of fluorescent spectra, which seems to be equivalent in many respects to Western equipment, has been described by Litvin and Krasnovskiy (1957). These workers have used this equipment (1958–1959) to measure the fluorescence of etiolated leaves and have obtained peaks in the fluorescent spectrum which correspond to those obtained from chlorophyll *a* in solution. Chlorophyll *b* produces no fluorescence from green leaves, and therefore they consider that there is an energy transfer from chlorophyll *b* to *a*.

Yaglova (1958) has described a technique of measuring the resistance of cells at high frequencies to determine their ionic conductivity without the

attendant difficulties of polarization effects and membrane resistance, which exist at low frequencies.

Energy Transfer

The Soviet work in the field of energy transfer, both theoretically and experimentally, is at a very high level and reflects the close ties between this subject and the physical sciences. There exist numerous reviews of Russian work in this field, which are distinguished from reviews in other fields in cellular biophysics in that a large amount of the work reported upon is of Russian origin. Konev (1957b) has investigated the polarization of the fluorescent radiation from red algae and has evaluated the relative quantum yield as a function of wavelength. From such measurements he has shown that there is an intramolecular migration of energy from the dye phycoerythrin to other chromophores. Krasnovkiy (1959) has reviewed the subject of the fluorescence of chlorophyll as related to photosynthesis. From fluorescence measurements he concludes that it is an oversimplification and a neglect of biochemical aspects to use a semiconductor model for photosynthesis. It may be that radical ions are formed instead of trapped electrons. These radicals are formed by electrons which are transferred by intermediate enzyme and redox systems. They represent triplet states and were first proposed as intermediates in the light absorption act in photosynthesis by Terenin. The sensitization of the photoelectric effect in substances such as zinc oxide by adsorbed chlorophyll and its derivatives is discussed by Putseiko (1959). The energy transfer from the pigment to the electrons in the semiconductor is thought to be a transfer to traps near the semiconductor surface. The action spectrum for this sensitized photoconduction is similar to the absorption spectrum of the chlorophyll in dilute solutions but is displaced 50–100 Å toward the red.

The transport of charges in systems of organic molecules is the subject of a review by Terenin, Putseiko, and Akimov (1959). They have determined the sign of the carriers of charge in crystals of pigments such as chlorophyll, and the positive sign which is found implies an oxidation-reduction intermolecular exchange of electrons at the ground energy states. They have also indicated that there is a light-induced exchange of protons in layers of amphoteric acridine derivatives as interpreted from fluorescence at low temperatures.

Reviews of experiments on mechanisms of energy transfer via a process known as inductive resonance or sensitized fluorescence are given by Vladimirov (1957b). Synthetic dyes were adsorbed to carbon monoxide hemoglobin. The irradiation of the dye-hemoglobin complexes by energy absorbed in the dye leads to a dissociation of the carbon monoxide from the hemoglobin. This is a result which is analogous to those in which energy absorbed by the aromatic amino acids results in dissociation of the carbon monoxide. All the above papers are at a high technical level, and most of them contain sufficient details that the experiments can be reproduced by other people.

Fluorescence Lifetime Measurements

Dmitrievskiy, Ermolaev, and Terenin (1957) have described experiments on the determination of fluorescence lifetime of chlorophyll with instruments whose resolving power is about 2×10^{-11} seconds. They find that as the pigment increases in chloroplasts, the half-life of the fluorescence becomes larger. This work is similar in experimental details to those performed by Brody in this country.

In a long series of papers, Gachkovskiy (1958, 1959, and earlier) has investigated the fluorescent spectra of various forms of chlorophyll and chlorophyll derivatives. Gachkovskiy's thesis, derived from his studies, is that Rabinowitch in the United States is wrong. He feels that the fine structure found in some cases, and the differences between the fluorescent spectra of adsorbed chlorophyll and chlorophyll in vivo, as compared to chlorophyll in solution, are the result of different types of complexes and do not arise from extraneous physical features such as light-scattering. Gachkovskiy's views have been attacked strongly in the Soviet Union by Evstigneyev (1959), who claims that "Gachkovskiy's own data, which he claims to support his views, are contradictory." They have also been criticized by Terenin (1958), who attacks Gachkovskiy because he does not believe in triplet states and because he implies that the conjugation of porphyrin rings is not important in their spectra.

Spectral Changes in Photo-Products

A large amount of work in this field is associated with the name of Evstigneyev, which appears on more than one-third of the papers. By spectrochemical techniques Evstigneyev (1957) has shown that chlorophyll and its analogues are photoreduced in solution via extremely labile and potentially electrode-reactive reduced pigment forms

with free radical properties somewhat like those of ionized semiquinones. Evstigneyev and Gavrilova (1958a) have investigated these intermediates by using low temperature techniques. The relations among the photoreduction of chlorophyll and the photo-oxidation of other substances which are present have been discussed by Brin (1957). He feels that the reaction rate of oxidation depends on the state of chlorophyll and that the rate is highest when the chlorophyll is monomeric. Brin and Krasnovskiy (1959) have followed up earlier observations on the reduction of diphosphopyridine nucleotide (DPN) in chlorophyll solutions and have showed that the reduction only takes place when a substance such as ascorbic acid is present; but that the reduction of DPN can take place without the oxidizing agent if homogenates of plant leaves are present. If the cytoplasmic granules are broken, however, the reduction cannot take place without the presence of ascorbic acid. Heating also destroys the oxidizing ability of the homogenates.

Evstigneyev and Gavrilova (1958b) have measured the oxidation of methyl red as sensitized by chlorophyll *a* in the presence of H_2O or D_2O. There is little or no difference with heavy water. Unfortunately, a few data are presented, although there is much discussion.

Structure of Chloroplasts and Chlorophyll

Godnev and Akulovich (1958), from investigations of the spectra of leaves at different temperatures, have determined whether chlorophyll is bound to lipid or protein. As the temperature increases the spectrum changes, and this change takes place when the protein is denatured. In order to reconcile data on the monomolecular distribution of chlorophyll in granular discs (the grana), data on the depolarization of fluorescence in chloroplasts of algae, and data on the size of the photosynthetic unit as determined by Emerson and Arnold in the United States, they propose a model of chlorophyll adsorbed to the interface between lipids and proteins with numerous bends in the chlorophyll layer. More recently, Godnev, Rotfarb, and Shlyk (1960) have indicated that C^{14} initially present in chlorophyll *a* is later on found in chlorophyll *b*.

The chemical composition of the proteins of chloroplasts has been investigated by Sisakiyan and Melik-Sarkisian (1956), who have shown that there are two RNA-containing nucleoproteins, two chromoglobulins, and a protein containing a large number of sugars. The techniques used were electrophoretic mobility, nitrogen and phosphorus determinations, and nucleic acid determinations by chromatography and spectrophotometry.

Miscellaneous

The relation between photosynthesis and phototaxis has been investigated by Babushkin (1955); and Maksimova (1957) has studied the effects of different wavelengths on development of purple bacteria. The action spectrum for the development of purple bacteria has been obtained. The absorption by carotenoids gives rise to photosynthesis with about the same efficiency as does absorption by bacterial chlorophyll. However, Maksimova gives no good description of the wavelength bands used.

5. EFFECTS OF ULTRASONICS ON CELLS AND CELL COMPONENTS

The techniques which have been used by the Russian workers are equal to those found anywhere, as is to be expected, since this is a problem involving physics. In most of the work reported there are at least estimates of the intensities of the sound waves, which is more than can be said for a good deal of the Western work. Descriptions of general techniques, equipment, and possible uses are given by El'piner and Dvorkin (1958), and particular machines for irradiating tumors are described by Burov (1956).

Molecules

The propagation of sound waves in aqueous solutions of muscle proteins has been described by El'piner and Pyshkina (1959). These investigators show that the difference in velocity between the solution and the solvent yields a measure of the immobilized water (water of hydration) associated with the proteins, and that the hydration increases with temperature. The reasons for the increase are not clear. El'piner and Sokol'skaya (1958) and El'piner, Deborin, and Zorina (1959) have described some effects of ultrasonic waves on amino acids and proteins. They have used 700kc/sec radiation at about 8 watts/cm². The irradiation of aliphatic amino acids gave rise to products which showed absorption maxima, in alkaline solution, in the region of 255 mμ. These authors hypothesized that the absorption arose from the formation of oximes produced by the action of the HO_2 radical, as in the indirect effects of ionizing radiation. The effects of various dissolved gases on the irradiation of proteins are somewhat similar to those

obtained for the irradiation of proteins by ultraviolet light in the presence of various gases. In oxygen the molecular weight goes down, while in hydrogen it increases. It was supposed that the effect of ultrasonics was to split the proteins into free radicals which, in the presence of hydrogen, react with one another to form large aggregates, in a manner similar to that proposed by Rideal and Roberts for ultraviolet light. Obviously, however, the exact mechanisms are not known. In connection with these papers it is worth mentioning that Levinson and Kovrov (1959) have measured the oxidizing ability of irradiated distilled water and that Khenokh and Lapinskaya (1951) have measured the production of aldehydes in the irradiation of many different types of biochemical material.

Zbarskiy, El'piner, and Kharlamova (1951) report the depolymerization of DNA by ultrasonic waves and have studied the various aftereffects on DNA and on the bases of DNA after irradiation. Many of these reactions are similar to the oxidations observed with other types of radiation.

Microorganisms

El'piner (1955, 1956) has reviewed the effects of ultrasonics on microorganisms. The killing and mutation effects of ultrasonics on microorganisms depend upon the shape of the organism. Thread-like organisms are much more sensitive than rods, which in turn are more sensitive than staphylococci or streptococci. Low temperatures during irradiation increase survival, perhaps because of a decrease in cavitation. There is an oxygen effect for cells just as there is for molecules. Low oxygen pressure decreases the indirect effect from oxidizing substances. El'piner thinks that there is probably no cavitation inside of microorganisms, but that the major effect is a breakage of the cell wall. Therefore the observed results depend, to a large extent, on the structure and form of the membrane. This result of course does not necessarily explain how mutations arise.

Ultrasonics may possibly be used to liberate surface enzymes such as invertase on the yeast cell wall (El'piner and Dvorkin, 1957). El'piner and Dvorkin (1958) have investigated the effects of ultrasonics on red cells by following the migration of red cells in an electric field after irradiation. From the migration rate they compute the zeta potential of the cells and show that it falls if the red cells are in an acceleration antinode of the radiation field but not if they are in a pressure antinode. Since the intensities in the experiment were too low for cavitation, they guess that the sound produced an effect on the ions on the surface of the cell. The experimental technique used is good, but there seems to be no real fundamental reason given for such an experiment. El'piner and Sheinker (1946) reported the extraction by ultrasonics of the endotoxin from whooping cough bacteria. The endotoxin lost its toxic properties on standing but kept its immunological property.

Large Systems

Breslavets and El'piner (1956) have described the action of sound on the cells of the fern *Proteallia*. They have recorded the cytological changes which take place with changing intensity. For example, the chloroplasts lose color at intensities of 2 to 3 watts/cm² for 30 min. At 15 watts/cm² the cells are completely dispersed. From these and similar observations, they conclude that ultrasonics works by a different mechanism than other physical agents such as ionizing radiation. Ultrasonics probably produce most of their action by liberating, or shaking loose, complexes of enzymes and cell parts, such as chloroplasts or invertase on cell surfaces, which are originally either undetectable or integral parts of the cell. In this way they produced general disorganizations of cellular structures rather than particular effects on particular molecules.

Tsareva (1957) has shown that sound waves decrease the mitotic rate in corneal epithelium, as do other types of physical agents, and El'piner and Bronskaya (1959) have described the effect of ultrasonics on the stimulation of germination in seeds, a matter that might be conceivably of practical importance.

6. PARAMAGNETIC RESONANCE

In the reviewers' opinion, some of the best of the Russian work under review is that on paramagnetic or electron resonance (ESR). This work, which is described by Blyumenfel'd (1957a, b; 1958), Blyumenfel'd and Kalmanson (1957a, b; 1958a, b), and Kalmanson and Blyumenfel'd (1959), is of a caliber which seems above that in Western countries. Good quantitative data are presented indicating that the physical part of the experiments is excellently performed, and moreover that the control of some of the biological systems is well taken care of. The descriptions of the work are usually excellent.

Blyumenfel'd and Kalmanson found that doses of 10^7 r given to dry amino acids gave rise to 10^{19}

free radicals per gram. The irradiation of proteins, on the other hand, gave a much lower ESR signal than expected from the sum of the individual amino acids, lower by a factor of 10^3 to 10^4. These latter results are in disagreement with those obtained by Gordy et al. in this country. Another difference between the patterns of irradiated amino acids and proteins is that the amino acids have very broad ESR patterns (50 to 100 gauss wide), whereas irradiated proteins show a narrow line whose width is about 10 gauss.

If proteins are heat-denatured before irradiation, the ESR line becomes broader and the yield of free radicals rises by a factor of 10^2 to 10^3. Kalmanson and Blyumenfel'd used cobalt γ-rays, and they feel that the difference between their results and Gordy's is that Gordy used a soft X-ray machine as a radiation source which, at the doses delivered, might have heat-denatured the samples. Their explanation for the difference between denatured and native proteins is that there is electron migration through the peptide-hydrogen bond structures or in general through the secondary structure, so that there are very few free permanent free radicals formed by radiation. In the denatured form the free radicals are trapped.

Blyumenfel'd and Kalmanson (1957b) have also investigated the ESR signals from enzymes and have shown that the band disappears upon inactivation of the enzyme. They feel that these data indicate a free electron conduction in native protein, as mentioned above.

In a rather long and tedious review, Blyumenfel'd (1957b) analyzes the theoretical basis of ESR and analyzes the narrow ($\sim$1.5 gauss) signals which are found in *unirradiated* lyophilized tissue. He thinks that the signal arises from delocalized electrons in proteins. In normal proteins the energy gap between the filled ground state and the conduction band is so large ($\sim$3 ev) that a negligible number of electrons will be found in the conduction band. However, says Blyumenfel'd, if a prosthetic group, or similar substance, is bound to the protein, it might transfer an electron to the protein. Since the lowest band is filled, this extra electron must go into the conduction band formed along the peptide and hydrogen bonds.

7. MICROORGANISMS

Genetic studies of microorganisms are still mainly dominated by Lysenkoism. However, opinions range from those of Prozorov (1959), of the Department of Microbiology, Moscow Medical School, who writes respectfully of Western work on bacterial recombination, to that of Timakov and his school (Gamaleya Institute) (1952, 1959a, b), who apparently believe that the Western world is behind in this field because of its prejudice against "directed variability," which means changing the genetic constitution by altering assimilation or other environmental factors. Thus *E. coli* cultured on autoclaved paratyphoid B can acquire characteristics of the latter organism (and the autoclaving presumably eliminates the possible effects of filterable forms). Likewise they claim that it is possible to produce non-virulent strains of pathogens by culturing them in the presence of immune serum, antibiotics, or even phages. So little experimental detail is given in the descriptions of this type of experiment that it is usually impossible to determine from the published work just what has been done or what occurred. Sluvko (1957) varied the experiment slightly by adding *Salmonella* antigen to leucocytes and *E. coli*, to produce a new type of bacteria with *Salmonella* antigens. Galkina (1956) measured the change in DNAase activity in *E. coli* mutants supposedly produced by culturing *E. coli* with *Bacillus breslau* antigen. A comment by one Soviet scientist at a "directed variation" conference indicates how universal are certain human difficulties: "the copiousness of fixed speeches is a real shortcoming . . . of the conference."

Another variation in the "directed variation" experiment is the use, for the killed strain, of bacteria radioactively labelled with S^{35} or P^{32} (Vasilenko and Belykh, 1958). The S^{35} appears in the live strain relatively early (indicating a transfer of protein components), whereas the P^{32}, presumably in the nucleic acid components, becomes part of the live bacteria much later. There seems to be a confusion in the Soviet literature between real mutation and adaptive enzyme formation (as in the paper of Kosikov and Raevskaya, 1959, on yeast fermentation).

Grinbaum and Kudlai (1957) report on a conference at which it was concluded that "particular attention should be paid to transforming principle, nucleic acids, bacteriophage, mutagenic factors, etc., capable of inducing variation in the hereditary constitution of microorganisms."

Many of the Soviet microbiologists apparently believe in filterable forms of almost all bacteria. Hauduroy (1957), although not a Russian, has written a review of this subject for *Priroda* in which

he explains their point of view. Often the Soviet writers take their point of view so much for granted that the outsider can hardly figure out what it *is*. The phenomenon described is the growth of "secondary cultures" in the filtrate of a suspension in which bacteria have been growing. The original bacteria supposedly cannot pass through the filter, and there is "regeneration" of bacteria from other forms. Sometimes bacteriophage are introduced, as in the experiments of Shchukin (1957), who lyses the *E. coli* culture with phage, and then observes "secondary growth" later, with new bacterial forms (he doesn't even present adequate proof of the sterility of the phage preparation introduced). Some authors believe in an intermediate granular stage, microscopically visible, between the filterable stage and the normal bacteria (Bosh'ian, 1949). Bosh'ian apparently thought in 1949 that bacteriophage were "bacteria made invisible." Sosov (1950) believed a toxin could be a filterable form of a bacterium!

There are a number of papers on the action of temperature, pressure, freezing, drying, etc., on microorganisms. Udel'nova has found evidence that yeast cells subjected to freezing and thawing are damaged mechanically by ice crystals (young cells more so than old ones). Disintegration after drying occurs upon rehydration (Udel'nova, 1957). Poliansky (reported by Vorobiev and Zhirmunskiy, 1958) showed that it was possible to develop temperature resistance in protozoa by changing the growth temperature. Bychkovskaya (1955) found that dysentery bacilli which were more resistant to sulfanilamides were also more resistant to drying. Evreinova, Ermolaeva, and Gerasimova (1958) have shown that the amount of nucleic acid base in a thermophilic strain of *Bacillus mycoides* is lower when the organism is grown at higher temperatures. Kriss, Biryuzova, and Abyzov (1958) describe a bacterium which grows at a pressure of 550 atmospheres. Chumak (1959), working on this same organism, claims that more glucose is consumed per unit length when it is under greater pressure. Bacterial filaments are formed from 550 to 750 atmospheres pressure.

Popova (1952) has evidence that binding of sulfhydryl groups has to do with sensitivity of paramecia to electric currents. Smirnova (1957), of the Moscow State University, confirms this result with the observation that glutathione decreases galvanotaxis and cysteine restores it. Koshtoyants and Kokina (1957) also conclude that galvanotactic sensitivity depends on metabolic state. Acetylcholine and anticholinesterase increase sensitivity in small concentrations, decrease it in large.

The resistance of paramecia to lethal salt concentrations depends on the concentration where the organism *has* been, a higher than normal concentration conferring resistance increased by a factor of three to very high concentrations (Seravin, 1958, Leningrad State University).

Vinogradov (1957) finds that the isotope K^{40} in normal concentrations has no effect on the growth or development of *Aspergillus niger*.

Microorganisms are widely used in the Institute of Antibiotics in the search for anticancer drugs (Gauze, 1958, 1959, 1960; Gauze, Kochetkova, and Vladimirova, 1957a, b). They claim that antibiotics which selectively kill respiratory-deficient mutants of yeasts and bacteria are more likely to kill "malignantly altered cells." Gauze and his group have found a very interesting correlation between the antibiotic production of a given group of organisms and their geographic derivation.

In general, the drug-resistance studies are characterized by lack of appreciation of the occurrence of spontaneous mutation in microorganisms. Thus Lebedeva and Voropaeva (1957), of the Department of Microbiology, First Moscow Medical Institute, discuss stepwise *adaptation*, and their failure to refer to or appreciate Western work on this problem makes the research they describe seem many years behind the times. This is also true of papers by Khazanova (1957) and Moroz (1956).

A series of papers by Spirin and his collaborators, on macromolecular components of microorganisms, are among the soundest covered in this review. These range from the strange report that during development of *Actinomyces* mycelium the DNA disappears, to be replaced by another nucleic acid containing a different base, possibly a precursor of the DNA (Demianovskaya and Belozerskiy, 1954), to nucleic acid base analyses in nineteen bacterial species, showing a possible correlation between DNA and RNA. The latter is an extremely important result, if it turns out to be correct (Spirin, Belozerskiy, Shugaeva, and Vanjushin, 1957). Zaitseva and Belozerskiy (1957) have found a decrease in amino acid content of *Azotobacter* during the latent phase of the organisms. The relative amino acid contents of different strains have been investigated by Blokhina, Perova, and Labrovskaya (1956) and Mitina and

Rubin (1955). Evidence of complexing of polysaccharides and nucleic acids in paratyphoid bacteria has been presented by Rukavtsov and Shubitch (1956). Oparin, Gel'man, and Zhukova (1955a) have discussed the latent stage of the enzyme invertase in the membrane of the yeast cell, as opposed to the active stage of maltase, which is present throughout the protoplasm. Petrov and Nosova (1956) used radioactively labelled methionine to show that methionine-requiring mutants of *B.* (*E.*) *coli* or *B. breslau* take up more of this substance than normal organisms, unless vitamin B_{12} is present. Phasing of cell division is noted by Erucalimskiy (1955), of the Kiev Clinical Hospital, when yeast cells are first placed in liquid nutrient medium. He measured gas formation, which decreases just before budding and then increases.

Protoplast studies are relatively crude. Kalyna's (1958) review presents only Western material. Gorodskaya and Miagkaya (1958), of the Department of Cytology, Institute of Experimental Biology, Moscow, have tried to measure respiratory activity in protoplasts (and found *none* in those of *B. megatherium*). It is concluded by Oparin, Gel'man, and Zhukova (1955b) that enzymes must be spatially located in protoplasm, since changing the sucrose concentration, which alters protoplast structure, changes glycine incorporation.

Fluorescence methods of counting microorganisms are used by a number of investigators. Razumovskaya and Osipova (1958) used acridine-orange fluorescence to count the number of dead cells in *Acetobacter* cultures of various stages. The percentages ranged from 4 or 5 per cent dead cells in the actively growing period to 70 per cent in late stages. A well-written account of the fluorescent antibody technique of determining small amounts of specific bacteria in the presence of large amounts of other bacteria is given by Dashkevich, D'iakov, Ermakov, Ivanova, and Maiboroda of the Kirov Military Medical Academy (1959). Details of technique are presented.

8. VIRUSES

The virus literature is one of the poorest among all the fields covered in this review. There is a notable lack of experimental work on virus structure, although no lack of speculation. Many of the investigators seem unacquainted with most of the important Western work of the last twenty years. Among the better-read workers in the bacteriophage field are Timakov and Petrovskaya (1957) and Zhukov-Verezhnikov and Pekhov (1957a), whose reviews of lysogeny and bacteriophage in general contain almost no mention of any Russian experimental work. The latter authors have a semi-philosophical outlook common among Soviet phage workers. It communicates very little.

Phage Electron Microscopy and Light Microscopy of Lysis

These fields are dominated by Pekhov (Institute of Experimental Biology, Academy of Medicine) and by Kriss. The Pekhov group (as in Pekhov's paper, 1957a) does not remove cellular debris or even whole cells when pictures of phage are taken. Pekhov has tried to study intracellular development of bacterial viruses by microscopy. He claims (Pekhov, 1957b) that some infected cells show cytoplasmic granules (mitochondria). His pictures of bacteria show a central nuclear body after OsO_4 fixation and thin sectioning, similar to that to be seen in pictures taken by Western workers. In one of Pekhov's light microscope studies (Pekhov, 1958a) he described lysing of an *E. coli* culture by phage 1321. Some of the cells, however, merely lost opacity and did not burst. Some lysed very slowly. He may possibly have been using a mixed virus culture. Pekhov gives no references after 1951 and gives *no* reference to Weidel, one of the most important workers in this field.

Kriss (1944, 1953) and Kriss and Tikhonenko (1952, 1953, 1957) have investigated the effects of high pressure on the structure of the bacterial viruses as revealed by electron microscopy. Kriss and his associate report objects in bacterial viruses not seen by anyone elsewhere. They seem to believe that phages are made of spirally twisted chains of globular proteins, which may be untwisted by high pressures. Kriss does not believe in ghosts of bacteriophage (phage protein coats without the nucleic acid inside, described by many Western investigators). He thinks that Western pictures of ghosts are the result of bad contrast. Tikhonenko and Kriss (1957) also claim that phage particles can attach by their *heads* to the host organism.

Maiboroda (1956), who has done electron microscopy on dysentery phages, also believes that the viruses are aggregations of globular proteins. Rautenshtein (1953) has published on electron microscopy of actinophage and Khuan-Lo (1958c) on *Salmonella* phage.

Phage—General Studies

Among the better biological phage studies are those of Khuan-Lo (1958a, b, and c; 1959) of the Gamaleya Institute. These are mostly categorizations of attachment rates of different phages, their latent periods, ability to attach to heat-killed bacteria, sensitivity to ultraviolet light and heat, and antigenic properties (neutralization rates being determined according to standard Western methods). Khuan-Lo makes the generalization that high lytic activity goes with high antigenicity, as does Peremitina (1957), who claims that dysentery phage titer is higher on cells with more antigen (this may be only the result of different attachment rates). Pospelova, also of the Gamaleya Institute, has done some sound but also routine work on diphtheria phages (1958a, b). Rubashkina and Kazakova (1958), of the Saratov State Public Health and Epidemiological Station, have used *Salmonella* and *Shigella* phage assays to indicate the presence of appreciable amounts of the bacteria in milk.

Some of the papers contain results that are considered too obvious and well-known to warrant publication in the West, such as Borisov's (1959) data showing that the phage yield is higher when the dysentery bacteria are actively growing, or the observation of Timakov and Goldfarb (1956) that the phage yield depends on the time after infection and the number of bacteria. Mamatsashvili's (1957) paper on *Brucella* phages gives some evidence that he is not aware that the age of the bacterial culture affects the phage yield. Zhukov-Verezhnikov and Pekhov (1957b) have claimed that on agar *E. coli* grows toward *Sarcina lutea* in a symbiotic relationship, possibly mediated by phage. However, these authors have observed bacterial growth in an initially empty microscope field, which is interpreted as growth toward the *Sarcina*. An unidentified *E. coli* phage is discussed by Vorobieva (1957), who finds that addition of glucose increases the phage yield and that electrophoretic studies indicate that certain proteins in the bacteria are broken up during the infection process.

In connection with the problem of utilization of phage in curing disease, Kvesitadze and Mikhailova (1957) have studied antibody production in cattle by phages. Antibody production can be followed after subcutaneous or intravenous injection, but is not found when phages are administered orally.

Genetic Studies Involving Phages

Many of the Soviet workers have apparently read about lysogeny, transduction, and transformation; and there is a tendency to describe genetic changes in microorganisms, when phages are present, in terms of one of these mechanisms, without establishing any experimental evidence for the real mechanism, as in the work of Alikhanian and Il'ina (1958a, b). Some investigators (such as Svet-Moldavskiy, 1957) have not even shown that phages are really responsible for the variation in the bacteria attributed to them. Likewise Pekhov (1958b, 1959) claims that filtrates of *E. coli* and *Sarcina* which have been grown together contain forms which regenerate new bacteria, and that phage is involved, in that some of the new cells lyse. He has no real phage detection methods, and his techniques are so sketchily described that the evidence for filterable new forms cannot be evaluated. Kats-Chernokhvostova (1953) discusses a typhoid fever latent (presumably lysogenic) phage which is said to prevent the O-antigen from developing. Chernomordik (1957) describes "pseudobacteriophagy," or the occurrence on seeded plates of plaques which look as though they were produced by phage, although no phage is present; and the author concludes that the spots involve a type of mutation. There is no work on phage genetics as such. Rautenshtein (1954), of the Institute of Microbiology, considers that much of the work in this field by Hershey and Luria is nonsense, and he appears not to understand what their work is about. He believes, contrary to the Western workers' evidence of mutation in phage within the host cell, that phage changes represent "adaptive variation."

The effects of antibiotics on bacterial viruses are discussed by Krasil'nikov and Kafanova (1957). Some viruses are affected by antibiotics, some are not.

Animal Viruses—Electron Microscopy

A study of the development of insect polyhedral virus inside the host cell has been done by Gershenzon (1955; 1956a, b and c, and d; 1957), who makes a good case for the liberation of virus from the polyhedral bodies before cell penetration. Draganov (1956a, b; 1957) has tried to follow the intracellular development of influenza virus and fowl pox virus; and he distinguishes between the round elementary form and the elongated vege-

tative form. Skalinskiy (1958) likewise shows electron micrographs of what he claims is developing pigeon pox virus. One worker, Gandziy (1956), of the Laboratory of Etiology of Tumors, Kiev, believes that his pictures show virus particles in preparations of chicken sarcoma.

Animal Viruses—General

The ideas of a number of older workers, in whose papers Engels is extensively quoted, still linger in this field. Viruses and bacteria are regarded as interconvertible; and the basic biological unit is not the cell, but the particle in the cell, which can, upon occasion, form a cell (Bosh'ian, 1949). Zil'ber (1952), who mentions the necessity of holding to Stalin's emphasis on environmental factors, discusses various experiments on the growth of several animal viruses on yeast cells. There are no reports of attempts to rule out contamination in these experiments, and thus they cannot be taken seriously. A few routine researches are presented, such as Andonov's (1957) study of the action of various agents on encephalitis virus, and the work of Morozov and Korol'kova (1953) on the action of specific serum on vaccinia virus.

Gauze (1957) claims that various antibiotics affect the development of neurotropic viruses inside cells. At the Antibiotic Institute antibiotics are screened for their effects on influenza, for example, by testing them on tobacco mosaic virus, since substances which are effective are likely to affect *both* the animal and the plant virus.

Plant Viruses

Sukhov and his collaborators are the authors of most of the papers in this field. They all are saturated with dialectical materialism. For example, these investigators believe that new species of plant virus can arise as a result of various environmental factors alone (Sukhov, 1953). Sukhov and Kapitsa (1956) claim that when potato and tobacco mosaic virus multiply in the same cell, new types of virus emerge (an observation not reported by Western workers). Interference properties as well as presence of intracellular inclusions are used to characterize the new types. Elevated temperature is also said to lead to variations in tobacco virus (Kapitsa, 1955). This group of workers has also done some electron microscopy on the development of tobacco mosaic virus, with no reference to results obtained by other groups, whether in the Western world or elsewhere. Because of their finding of particles of different lengths early in development, they conclude that the virus increases lengthwise (Sukhov and Nikiforova, 1955a, b). Gol'din and Vostrova (1959) describe a new form of TMV which produces filamentous inclusions in the form of a ring in the cell nucleus. Smirnova (1955; 1958a, b) also followed TMV development by means of electron microscopy, and found particles even in non-necrotic zones of the plants. The meristem regions of vegetative buds showed no mature virus, although the disease first appears in the youngest leaves.

The effect of various substances on the growth of TMV is usually not correlated with any mechanism (Sukhov, Solov'ev, and Nikiforova, 1953), although Ryzhkov and Marchenko (1957) guess that the effects of sulfanilamide involve the folic acid enzyme system, since folic acid removes sulfanilamide action.

Ryzhkov (1957) has written a long review of experimental attempts to elucidate the development of tobacco mosaic virus by adding various kinds of inhibitors to the leaves of the plant. He believes that in most cases substances that are applied to the tobacco leaf and that suppress the multiplication of virus do so by affecting the cells, not by directly damaging the virus. He presents many different experiments of this type done by his group, and points out that one can find out in this way the difference between the needs of the virus and that of the host—for example, the virus will grow in starved leaves and anoxic leaves.

There is some skepticism about the validity of Western work. For example, Klein (1956) does not accept all the work of Fraenkel-Conrat and Williams on the separation of TMV into protein and nucleic acid and the reconstitution of the virus from these parts. He advances the hypothesis that the results could be explained by assuming that TMV has invisible forms, just like those postulated for other microorganisms.

Spirin, Gavrilova, Bresler, and Mosevitskiy (1959), of the Bakh Institute, have actually measured a number of physical properties of TMV ribonucleic acid (optical activity, viscosity, sedimentation, and light-scattering at different temperatures) in an attempt to elucidate the virus structure. This is one of the few examples of genuine molecular biology in the Soviet literature.

9. CELL STRUCTURE

There are many Soviet biologists who do not subscribe to the theories of Lepeshinskaya (1950, 1953) on cell structure and the origin of cells from

non-cells. (In her writings Lepeshinskaya strongly advises her fellow scientists to follow the teachings of dialectical materialism, and in particular Stalin's interpretation of them). Viazov (1952), however, continues to believe in the Lepeshinskaya fantasy. He refers to the reactionary theory of the continuity of the germ plasm. The Lepeshinskaya school maintains that in many biological systems the cell nucleus disappears and is reformed at a later time. The history of this controversial topic has been well and critically reviewed by Zhinkin and Mikhailov (1955a, b; 1958). It is sad to note that Oparin, whose book on the origin of life has been widely admired in the United States, has also supported the Lepeshinskaya group. He has said (Oparin, 1950) that being "progressive . . . is possible only in the country where science does not recognize any dogmas."

Eliseyev (1955) has written a very long and rambling review of various aspects of the cell theory. Most of his conclusions seem to be based on inadequate cytological observations. Little experimental work is discussed. Eliseyev mentions that some investigators have applied the theories of Lepeshinskaya to the problem of cancer, and believe that cancer cells can originate in the body from non-cellular material. He does present some of the criticisms of Lepeshinskaya's experiments, with the following comment: "The history of science teaches that all that is true, that objectively reflects the laws of nature, is preserved and confirmed by other investigators, and serves the further progressive development of the knowledge of nature, while all that is erroneous, false, sooner or later is refuted, thrown out and soon forgotten by everybody."

The subject of cell structure and heredity seems to bring out the most uncritical and least objective in the Soviet biologists, as in the paper by Schmal'gauzen (1958), which contains some reference to information theory, and to what the Russians call "cybernetics," which could be translated as "silly genetics." This type of article contains no information whatsoever. Soviet geneticists are apparently divided into two warring camps: those who believe in "creative Darwinism" (akin to Lysenkoism) and those individuals who have been influenced by Western work, such as Dubinin (whose 1956 article shows acquaintance with the work of Benzer, Muller, Hershey and Chase, and the Watson-Crick DNA model). The first group is not enthusiastic about chromosomal inheritance, and its members argue that there are many other ways to transmit characters (called vegetative hybridization). Examples of these are the change in ear color from that expected from a rabbit's own genotype when a fertilized ovum is implanted in a female rabbit of another breed—the only two rabbits which survived were said to have the ear color of the foster mother—or the change in a few of a chick's feathers when its *parents* received blood from another breed (Neifakh, 1952). No controls are described in these experiments. But several Polish investigators (Ruus and Zelnin, 1957) were unable to produce any characters of the donor bird when some of its protein was inserted into the egg of another breed.

There is a lack of experimental work elucidating the relative role of nucleus and cytoplasm in the cell. A review by Kedrovskiy (1958) on this subject mentions Western work almost exclusively. He discusses at great length the inability of totally unrelated nucleus and cytoplasm (from two different organisms) to form a harmonious entity. Astaurov and Ostriakova-Varshaver (1957) made "hybrids" of the distantly related silkworms *Bombyx mori* and *Bombyx mandaria* by destroying one of the two—sperm or egg—pro nuclei (the two species will not normally form a hybrid), and found that the specific characters of the resulting organism are conditioned by the nucleus that remains. Sokolov (1955), of the Leningrad State University, has studied the grasshopper testis, where he has found fusion of nuclei, and there may also be fragments of cytoplasm inside the giant nuclei that result. He postulates that nuclear fusion is a defense mechanism of the cell, and has the effect of decreasing the nuclear-cytoplasmic interactions. This mechanism does not help the cell in the long run, however, since it nearly always degenerates at a later time.

Most of the studies of cell structure are histological (and descriptive), such as the work of Kiknadze (1957a, b; 1958). She has carefully described the change in the nucleolar apparatus of a copepod during oogenesis, and has found a difference in this process according to whether the animal has been kept in a pond or an aquarium. Konarev, Zakirov, and Elsakova (1958) have used the ability to take up stain as an indication of the state of deoxyribonucleic acid in the onion cell nucleus (for example, acid hydrolysis causes certain nuclei which do not normally do so to take up stain, and this is considered by the authors to be connected with the weakening of the protein-DNA bond). Raikov (1958) has estimated protein, DNA, and RNA during division of macronuclei

and micronuclei of ciliates, by means of staining procedures. The photographs in this paper are impressive. Ultraviolet microscope studies by Brodskiy and Nechaeva (1958a, b) and Brodskiy (1959) show that the amount of RNA in retinal ganglion cells increases during light stimulation, but the DNA remains constant during RNA synthesis. A blood supply is needed for the RNA increase to occur. Zhirnova (1958) claims that adrenalin decreases the amount of RNA and DNA in tissues of mice, as measured by alteration of staining. Fluorescence microscopy has been shown by Breivis and Vasil'ev (1959) to be a method of distinguishing between dead and living ascites cells, as measured by the ability to form a tumor in mice.

Biochemical studies of cellular structures are relatively crude, such as those by Zbarskiy and Georgiev (1959), of the Severtsov Institute of Animal Morphology, Moscow, who have fractionated rat liver nuclei. This type of work is apparently not so far advanced as that of Mirsky in the United States many years ago. Engel'hardt and Burnasheva (1957) of the Bakh Institute have separated out the spermosin of bull sperm cells. The ATPase activity is found mostly in the tails of the sperms. A typical "biochemical" experiment is that of Ermolenko (1957), of the Khabarovsk Medical Institute, who has counted the number of mitoses in the mouse cornea when various substances are injected, and finds that some have an effect, some do not. Although it is not in the least clear *what* is affected by these substances, Ermolenko draws conclusions as to the "role of nucleic acid and carbohydrate metabolism in cell division." Differences in the biochemistry of normal and tumor cells have been investigated by Alov (1959), who claims that RNA increases during division of tumor cells but not in normal cell division. Alov and Semenova (1958) report that regenerating salivary gland tissue in mice gives out a substance which stimulates mitosis in normal tissue. Zbarskiy and Perevoschikova (1957a, b, and c) present evidence of what they think is a difference in type of protein synthesis between normal and tumor tissue. Incorporation of labeled amino acids was higher in the nucleoprotein fraction of tumor tissue, and higher in the acid protein fraction of normal tissue.

An experimental approach to the problem of differentiation made by Neifakh and Rott (1959) in the pond loach gives the result that a departure from synchronous cell division, which is the first sign of differentiation, may be eliminated by exposure of the early embryo to low temperature (up to the middle blastula stage). Synchronization cannot be achieved in this way in the early gastrula stage. Irradiated embryos became asynchronous (and thus begin differentiating) at the same time as unirradiated ones. Differentiation of antigenic properties of the lens was investigated by Konyukhov (1956a, b; 1959), of the Laboratory of Immunology and Embryogenesis in the Institute of Experimental Biology, Moscow. He sensitized guinea pigs by lens suspensions from ducks, and measured shock reactions when duck serum was administered later. Species specificity was thus shown to be present in the lens rudiment of very early embryos.

The problem of cell permeability is tackled verbally rather than experimentally. Troshin (now deceased) wrote a book on this subject (reviewed by Nasonov, 1959) in which he presents the hypothesis that the coacervate properties of protoplasm are responsible for the adsorptive properties of cells, rather than the cell membrane. Trincher (1958) suggests that permeability is best explained by a model of a cell as a homogeneous ball containing protein dipoles, but that polarization properties of the cell can be explained either on the basis of this model or with the membrane theory. He measured electrical resistance of blood and serum over a wide range of temperatures and frequencies (this was apparently done twenty years ago, as a thesis problem). He concludes that since the insulating properties of blood decrease when the temperature increases, the membrane theory is refuted. Trincher also quotes the data of Maslov and Sverkkov (1950) on the change in polarization of muscle with salt concentration. These are said to support the dipole cell model. Tonus is accompanied by a decrease in both low frequency and high frequency conductivity, according to Aladzhalova and Mertsalova (1954), but normal contraction involves an increase in low frequency conductivity, with little change in the high frequency conductivity. Trincher regards these changes as further support for his theory, in that he believes them to be the result of changes in protein dipole moments.

Khanykova (1958) made the same type of measurements on the chicken egg. Whereas the ratio between high frequency and low frequency resistance remains constant for the white and for

unfertilized yolk, it changes markedly in the case of developing egg yolk, a phenomenon which the author considers to be a reflection of the underlying structural changes in the yolk. The effect disappears when the yolk is vigorously stirred.

There is almost no use of radioautography to investigate cell structure. One example of the use of this method is a study by Gracheva (1958) of P^{32} incorporation into nerve cells in comparison with that into liver cells. No general conclusions seem possible from this type of study.

Only a few scattered physical measurements have been made on cellular structures. Ellengorn and Svetozarova (1950) report the isoelectric points of plant chromosomes to be different at telophase and at anaphase. Koshtoyants and Kokina (1958) inserted microelectrodes into unicellular organisms to measure oscillations in potential which follow the initial increase. These oscillations depend on the tonicity and potassium content of the solutions, and are affected by metabolic inhibitors.

10. PROTEINS

The outstanding work in Russia on the properties and structures of proteins has been carried out by Orekhovich and his collaborators at the Institute of Biological and Medical Chemistry, Moscow, on the structure and property of procollagen. This work, and also that of Oppel (Kirov Military Medicine Academy, Leningrad) on peptide sequences in proteins, is equal to the most excellent biochemical work carried out in the West. Much of the other experimental work is concerned with subjects which are not emphasized in Western countries, and many of the interpretations made are beclouded by political motivations. Nevertheless, some of the strange experimental findings and peculiar experiments should be repeated.

Enzymatic and Other Protein Reactions

The general principles of enzymatic reactions and their specificities have been discussed by Balandin (1958) and Mosolov (1957). The first paper, which is verbose, attempts to describe enzymatic specificity in terms of a "multiplet theory" and relationships between bond energies and the fitting of substrates to enzyme surfaces. The discussion is in many places superficial or obscure, and to the reviewers, does not seem to lead to any useful conclusions. The second paper questions whether the specificities of proteolytic enzymes, which are derived from experiments that use low molecular weight synthetic substrates, are an adequate explanation of the data obtained upon hydrolysis of native proteins by such enzymes. There is no mention of the wide body of information concerning the use of proteolytic enzymes as reasonably specific reagents to determine amino acid sequences in polypeptides. In general, these papers show good evidence of familiarity with Western techniques but do not compare with Western investigations as far as reaction mechanisms are concerned.

There are numerous papers, some of which will be mentioned in the section on nucleic acids, on the relations between proteins and nucleic acids in multicomponent systems and in coacervates. These multicomponent systems consist of gum arabic, protein, and nucleic acids and, for example, are concerned with the influence of adsorbed nucleic acid on the activity of trypsin in such coacervates (Oparin, Deborin, and Baranova, 1957). Obviously the physicochemical properties of such multicomponent systems are very difficult to analyze. The motivation for their study is of general interest in relation to the origin of life and in respect to the particular reactions that preferentially take place at interfaces. No clear-cut generalizations seem to have emerged from this work on proteins and nucleic acid.

Other investigations of multicomponent systems are those of Kakuliya (1959), of the Institute of Physiology, Tiflis, who has investigated, by electroconductivity, viscosity, and ultraviolet absorption techniques, the interactions among serum albumin, glucose, ATP, and lecithin; and the work of Chernikov and Shpikiter (1955), who were concerned with reactions between trypsin and ovomucoid.

Gulyi, Litvinenko, and Dvornikova (1959) described interactions between aldolase and other enzymes. Muscle phosphofructokinase was activated by aldolase, but the authors said that this reaction was probably the result of aldolase removing hexose diphosphate, which inhibits the kinase. The interaction of aldolase and alpha-glycerophosphate dehydrogenase results in an increase in the activity of the aldolase but a decrease in activity of the latter. It is dubious that this arises from the screening of the "active center" by aldolase, as the authors suggest.

Much of the discussion of enzymatic reactions is repetitious of long-published literature, and some of the remarks are vague and useless. For exam-

ple, nothing is added by saying, as Chernikov (1958) does, that "enzymatic hydrolysis of native proteins cannot be explained as cleavage of denatured protein which is presumed to be always present in equilibrium with the native form." In the field of enzymatic reactions, however, the work of Kazakova, Orekhovich, and Shpikiter (1958a, b) on the action of the enzyme collagenase is a model of technique and presentation. Not only have they measured the temperature dependence of the reaction, by observing the change in the viscosity of collagen, but they have also determined the amino acid groups next to each break and have thus obtained partial sequence analyses of collagen.

On the more speculative side is the work of Bresler, Glikina, and Frenkel (1954) on the enzymatic activity of protein fragments which were obtained by treating trypsin and aldolase with trypsin at pH 9 (an unusually high pH for tryptic activity). Between 5 and 10 per cent of the peptide bonds were broken and the resulting hydrolysate was spun in the partition cell of a centrifuge. The top and bottom fractions were assayed for enzymatic activity (but no filter paper was used to prevent convection). The authors estimated that only molecular weights of 2000–3000 are left in the top compartment, but they still found a large amount of activity (around 20 per cent for aldolase and somewhat less than 50 per cent for trypsin) in the top of the centrifuge cell. Centrifugation of an unhydrolyzed preparation gives negligible activity in the top fraction. Obviously, the successful performance of such an experiment depends upon a lack of diffusion and upon the formation of a good sedimenting boundary which, if the preparations are not pure, may be difficult.

The effects of high pressure on the enzymatic activity of trypsin and chymotrypsin are described by Vorobiev (1958). Again, pH 9 was used. Vorobiev found that there was a much lower reaction rate at high pressure, but that the reaction rate was independent of the type of substrate. He mentioned compressing enzyme-substrate complexes and proteins, but there was no attempt to apply transition rate theory or to calculate the volume change of the activated complex. Ivanov, Mirovich, and Parshina (1959) describe the effect of high pressure on the ATPase activity of myosin. At 4000 atmospheres the activity is lost, and the authors reach the remarkable conclusion that "there must have occurred some kinds of changes in the last structure, these being associated with the inactivation of certain groups (possibly S-H)." The paper contains no real description of experimental details.

Structure and Composition

The structure and composition of proteins has been the subject of a relatively large number of papers and reviews; but, with a few notable exceptions, little attention has been paid to the precise determination of the amino acid order of proteins or to the physicochemical information, such as sedimentation constant, diffusion constant, viscosity, and flow birefringence.

The use of osmotic pressure measurements has been described by Alimov (1956) and applied to the protein sericin of silk. Surface properties of proteins are discussed by Deborin, Ivashchenko, and Smirnova (1957) and a general review of physicochemical techniques has been given by Tsvetkov (1957). The use of infrared absorption spectra in protein structure determinations has been the subject of a paper by Shigorin, Mikhailov, and Kliueva (1956).

Between the time of the review of Gavrilov (1953) on the theory of protein structure—in the opinion of the reviewers an elementary exposition—and that of Viazov and Tseitlin (1956) on antibody-antigen relations, there are eight general reviews on protein structure. Goldovskiy (1957), in a very repetitious review, has attempted to describe some regularities in the chemical structure of proteins. The verbosity is shown, for example, by the statement that

> in the case of the presence in a protein of several amino acids in large quantities usually one of them is present in maximum quantity in comparison with the others. In general, the amino acid composition of protein is characterized by the maximum content of the amino acid and by a smaller content of other amino acids.

This article emphasized the fact that there are different amino acids and, with aberrant exceptions, they are all α-L-types. There are only vague data on relative composition and practically nothing on the secondary or tertiary structures of proteins. The review ends on the note that proteins which were thought to be "pure," viz., casein, are not single components, and probably many other proteins will turn out to be heterogeneous.

Among the specific proteins which have been investigated are sericin, which contains mostly serine and whose amino acid composition has been given by Alimov (1956); and glycinin, investigated by Kretovich, Smirnova, and Frenkel (1956, 1958, 1959), who examined its stability and showed that the reversible reaction between protein subunits of molecular weight 120,000 and the dimer form depends on cysteine and ATP. Oppel and Serebrenikova (1958) have investigated the proteins of smooth muscle and by salting-out procedures have isolated eight different types.

Some of the characteristics of spermosin, the contractile protein of sperm cells, have been investigated by Burnasheva (1959). The protein was isolated by techniques similar to those used for myosin. It has ATPase activity and forms a complex with actin as does myosin, but no actin component has been isolated from spermatozoa. The protein is not affected, as is myosin, by ATP or Mg^{++} or Ca^{++}. This work seems to be of excellent quality.

Orekhovich and Shpikiter (1956) and Orekhovich, Lokshina, Mantiyev, and Troitskaya (1956) have investigated pepsinogen and pepsin and, from sedimentation and diffusion measurements, obtained for the molecular weight of pepsinogen a value of 42,000 and for pepsin one of 33 000. The N-terminal amino acid of pepsinogen is leucine and the C-terminal amino acid is alanine. The authors feel that pepsinogen consists of one polypeptide chain. Orekhovich and his group have also characterized the soluble proteins of the crystalline lens and showed by sedimentation-diffusion measurements, and by well-described, easily understandable procedures, that there are in the lens at least 5 to 7 soluble proteins whose molecular weights range from 14,000 to 800,000. They give axial ratios for these proteins.

Procollagen is probably the best and most thoroughly studied protein of any which has been worked upon in the Soviet Union. Most of the work has been carried on by Orekhovich and his coworkers in the years 1948 through 1959. The precursor of collagen, procollagen, was first found by them; and its physical chemistry and biochemistry, including the sedimentation and diffusion constants, molecular weights, and the number of components, as well as details of isolation, are well described. Procollagen has been found to consist of about 25 per cent glycine and 25 per cent proline and hydroxyproline. Its amino acid content is different from that of collagen, but experiments with labeled amino acids indicate that it is the precursor of collagen. The Russian workers feel that the protein called tropocollagen in the United States is the same as what they have called procollagen. The molecule has a length of 6,000 A, a molecular weight of 500,000 and, of course, produces very viscous solutions. [Earlier measurements by Bresler, Finogenov, and Frenkel (1950), which gave a very small value for the molecular weight, are believed to be incorrect because they were made on degraded procollagen.] The alpha and beta components of procollagen have molecular weights of 125,000 and 290,000. How these two components make up a molecule of procollagen, which should have a value of 500,000, is not clear. All this work on procollagen is obviously at a very high level of technical and theoretical competence.

Millionova and Andreyeva (1957a, b) have used X-ray diffraction and model-building to arrive at a structure for collagen which contains ten residues in three turns. Their model is in reasonable agreement with the experimental observations and may explain part, but not all, of the structure of collagen. The structure of the protein collastromin, which has been described as the reticulum which forms the base on which collagen is laid down, has been described by Tustanovskiy, Orlovskaya, Mikhailov, and Zaides (1954) and by Zaides, Tustanovskiy, Orlovskaya, and Pavliknina (1959).

The temperature dependence of the viscosity of collagen solutions has been described by Esipova (1957), and her data indicate that there is a very narrow temperature range in which the viscosity changes suddenly, as for solutions of DNA. She interprets the results to indicate that the hydrogen bonds in collagen are intramolecular, and so she favors Crick's model.

Oppel (1956, 1958) has used pepsin digestion of hemoglobin, followed by paper chromatography, to analyze for some of the peptides produced. He took the trichloroacetic acid (TCA)-soluble peptides (whose average length is 4 amino acids) and ran them on two-dimensional chromatograms. These gave somewhat more than 20 spots, of which 8 appeared in parallel runs. This work, which seems to be carefully and well done, gives the amino acid composition of these

8 peptides which contain between 3 and 8 amino acids.

The Stability of Proteins

The subject of the stability of proteins to various denaturing agents does not seem to be represented by a concerted and single point of view. Unfortunately, most of the work has been concerned with proteins which have not been adequately characterized, structurally or enzymatically.

In a very interesting paper, Aleksandrov (1952) has investigated the thermolability of cytoplasmic proteins in many different species. He has, ingeniously enough, measured the temperature at which the cilia of various species, from mollusks through mammals, stop beating. In all mammals the temperature at which the cilia stop is close to 46.4°C. In the rooster it is 49.2°C. In cold-blooded forms the temperature is determined by the *average* environmental temperature, not the instantaneous value. Species of frogs transferred from a warm climate to a cold one have the characteristics of the higher temperature even after 50 years. The lability differs from one species to another but, in general, not within a single species.

An example of the denaturation of non-specific proteins is that given by the work of Belitzer and Sayenko (1957) on the acid and alkaline denaturation of egg albumin as followed by viscosity and specific rotation. They found by these criteria, which are obviously inadequate for even an approximate specification of what is going on, that the reaction was reversible at room temperature and at the correct pH. A much better set of experiments has been described by Gorbacheva, Bresler, and Frenkel (1957), who were concerned with the relations among —S—S— bonds of serum albumin and chymotrypsin and denaturing agents. They reached the conclusion that

> lack of a disulfide framework facilitates denaturation and, unless the configurational changes are accompanied by the disruption of chemical bonds, restoration of the original structure can be effected upon removal of the denaturing agent.

As an example of an extremely poor paper in this field, we cite that of Tsyperovich (1956), which is entitled "The Denaturation of Globular Proteins." This long article is all qualitative and repetitious. It contains not one equation. The author worries for a long time about the definition of denaturation and yet never gives one. He finally, but implicitly, uses as a definition the abrupt change in properties such as appearance of —SH groups. He fails to associate optical rotation changes with changes in helicity of proteins, nor is he convinced that the changes in spectra arise from changes in the hydrogen-bonding of the —OH group of tyrosine. In a later paper, the same author (Tsyperovich, 1958) investigates the effect of nitrous acid on chymotrypsin and finds that one mole of nitrous acid inactivates one mole of chymotrypsinogen and that the solubility of the protein goes with its activity.

The stability of multicomponent systems or complexes has been investigated, among others, by Deborin and Shibanova (1955). They showed that the stability toward ultraviolet light, urea, or sound, of complexes of egg albumin and ergosterol was greater than if the egg albumin were not complexed. Kofman and Kalamkarova (1958) have described the effects of denaturing myosin on its ultraviolet absorption spectrum, but these changes are to a large extent influenced by the changes which take place in light-scattering. As a last example, we cite the work of Pasynskiy and Chernyak (1952) on the oxidation of —SH groups of proteins after urea treatment. They argued that urea weakens the —SH bond, thereby increasing its oxidizability. We suspect that most other investigators would think that the effect of urea is to open up the tertiary and secondary structure of the molecule by breaking hydrogen bonds and so revealing —SH groups which previously were hidden inside the molecule.

Incorporation of Amino Acids into Proteins, and Protein Synthesis

Work on the incorporation of amino acids into proteins and on the general problem of the specificity and kinetics of protein synthesis may be divided into two types. Type A resembles Western studies, type B does not. The former style of incorporation study is characterized by the fact that the proteins which are studied are well specified and that the synthesis and amino acid incorporation into these specific proteins are the objects of the investigation. Such work is that of Levyant, Levchuk, and Orekhovich (1959), in which the incorporation of cystine and methionine into the serum albumin of the rat and into the tissues of chick embryos was measured as a function of time. They found that the specific

activity incorporated into chicken embryo tissues was constant over time and that the incorporation into serum albumin only took place when serum albumin was synthesized. These data are interpreted to indicate that protein is made from amino acids at the time of formation of the protein, and moreover that there is no renewal of the amino acids of serum albumin once the molecule is formed. This work is contrary to that reported by Bresler and Kritsman (see below) and by Medvedev, who feels that incorporation takes place via peptides. In a similar way Orekhovich, Levyant, and Levchuk-Kurokhtina (1954) have shown that incorporation takes place only during the synthesis of proteins in the chick embryo. Orekhovich, Shpikiter, Kasakova, and Mazurov (1959) have followed the incorporation of C^{14}-labeled glycine into the alpha and beta components of procollagen.

In a series of papers by Khesin (1953) and Khesin and coworkers (1955, 1957), the incorporation of amino acids by the cytoplasmic granules of pigeon liver into amylase has been studied. It is found that this incorporation is efficient only if substances derived from mitochondria are present. All amino acids must also be present and, in this careful work, net protein synthesis has been followed by means of the biuret reaction, by TCA precipitates and protein nitrogen as well as by the uptake of radioactive amino acid. Sufficient data on the procedures are given to permit someone to repeat the work, even though no data are presented on the specific activities of the amino acids used. Similar descriptions of the involvement of mitochondria in amino acid uptake have been given by Sisakiyan (1956) for tobacco leaves. Protein synthesis in mitochondria has been described by Zubovskaya and Tongur (1959). They found that damaged mitochondria can only make protein if a cofactor from undamaged mitochondria is added. RNAase stops protein synthesis in damaged and undamaged systems.

Zbarskiy and Perevoschikova (1956) have shown that in malignant cells there is a large amount of nucleic acid synthesis, but that there is little incorporation of amino acids into the nuclei of such cells.

A typical example of the second style of incorporation experiment is that described by Konikova, Kritsman, and Samarina (1954). This experiment is concerned with the incorporation of amino acids into proteins in vitro. Amino acids containing C^{14} were incubated with various proteins and nucleic acid fractions at 37°C or 100°C. Determinations of bound amino acids were made by TCA precipitation. The authors say that the amino acids were incorporated, because dialysis failed to remove them. They used around 10 mg/ml of protein, and of the original 10^5 counts put into the solution about 10^2 were "incorporated" in two hours. The inclusion of cold amino acids in the mixture led to a decrease in the incorporation rate of the radioactive amino acid which the authors do not interpret as "competition" but as an indication of some strange type of reaction kinetics. There is no good evidence that the amino acids are "incorporated" by means of peptide linkages, although in a later paper by Konikova, Sukhareva, and Kritsman (1959) the authors claim that the bonds formed by incorporation in vitro are the same as those by incorporation in vivo. Such uncontrolled and poorly executed experiments have been criticized very strongly by other Soviet investigators, notably by Orekhovich, Kurokhtina, and Buyanov (1953), who think that the reported incorporation arises from a bacterial contaminant in the solutions. The binding of amino acids to proteins has been investigated by Shnol (1957), who finds that each 50–100 molecules of protein bind one molecule of amino acid, primarily at a C-terminal end. This is a very different type of reaction than the replacement of one amino acid by another.

The problem of the mechanism of protein synthesis has been investigated and reviewed by Medvedev (1955, 1956, 1959). Medvedev also attacks the work on the incorporation of amino acids in vitro mentioned above, on account of the non-sterile technique of the investigators, their incomplete presentation of data, and their failure to consider other reactions of a non-biological nature which may take place in the incubation mixtures. From a review of the experimental work on the synthesis of proteins, mostly carried out in the West, but also as indicated by the work of Bresler, mentioned below, Medvedev feels that protein synthesis takes place via peptides rather than amino acids. He thinks that the work of Haurowitz, in the United States, proves that nucleic acid cannot be the template for protein synthesis and that Bresler's work indicates that proteins cannot be templates either, because degraded proteins may still have enzymatic activity. The only remaining way is for the synthesis of proteins to come from big pieces which result from the hydrolysis of other proteins. It is these

large pieces, he thinks, that give continuity to life. This sort of an explanation supplies no feeling of how the number of molecules in a living system increases with time. In some passages, Medvedev is clear; in others, he typifies what might be called lyricism in scientific writing:

> There can be no strict separation of resynthesis and new synthesis. They interweave with one another and every resynthesis includes in it elements of regeneration, while on the other hand any regeneration of protein processes by resynthesis occurs with participation of large specific fragments.

Medvedev's experimental evidence for the use of peptides in protein synthesis comes from the use of hydrolysates of leaves and of pepsin digests of leaves, which have been labeled with C^{14}, as food sources for new growing leaves. He has investigated the relative concentration of C^{14} in the carbon dioxide produced by the leaves and finds that the leaves grown on the hydrolysate produce more $C^{14}O_2$ than the leaves which have been fed on the pepsin digest. He feels this indicates the involvement of peptides in synthesis.

Bresler, Glikina, Selezneva and Finogenov, (1952) have investigated the effects of high pressure on hydrolyzed proteins. Hydrolysates of serum albumin and insulin obtained by use of trypsin and chymotrypsin at pH 9 were put at a pressure of 6,000 atmospheres. Resynthesis of these hydrolyzed proteins was judged by a decrease in Van Slyke nitrogen, which the authors take to be an indication of reformed peptide bonds, and by the change in sedimentation pattern in an ultracentrifuge. After "resynthesis" the originally absent sedimentation peak appears, although it is much broader than the original one. There is no evidence that any specific protein has been formed, let alone any convincing evidence that peptide bonds have been reformed.

Other miscellaneous work on proteins is concerned with the structure and function of antibiotics. This is exemplified by the work of Silaev and Stepanov (1957).

It is fitting to close this section with a quote from an article by Pokrovskiy and Gorkin (1958), as given by Stekol, which is applicable everywhere, although it should be clear from what we have said above that in general research on protein structure in the Soviet Union is at the moment well behind that in the West.

> The existing system of education of scientific workers does not guarantee the implementation of biochemists with the necessary knowledge of physics, mathematics and physical chemistry, subjects which are essential for the successful study of protein structure.

II. NUCLEIC ACIDS

Relations between Nucleic Acids and Proteins

The work on multicomponent systems seems to be of a higher level than that in the protein field. There is particular interest in complexes between nucleic acids and protein. Blyumenfel'd, Kalmanson, and Shĕng P'eikĕn (1959) have studied the electron spin resonance (ESR) pattern of complexes between RNA and serum albumin. They find that the ESR signal does not change much, even after 5 to 10 minutes of boiling. The concentration of free spins changes rapidly with temperature, somewhat as in the curie transition in ferromagnetic substances. The authors conclude that

> the data show that there are enormous numbers of strongly interacting unpaired electrons in native nucleic acids, free or bound to proteins. Free electron concentrations are not very much lower than those found in metals.

Poverennyi (1959) has investigated properties of complexes of procollagen and bacterial nucleic acids. The susceptibility of procollagen to trypsin digestion is greater when complexed with DNA. The complexing ability is destroyed by heating the protein, and the combination of procollagen and DNA produces changes in the absorption spectrum. The stoichiometry of DNA-protein complexes has been examined by Diskina and Spitkovskiy (1958). They found that the weight ratios of chymotrypsin and bovine serum albumin bound to DNA are the same. The molar ratios are very different. Baranova, Deborin, and Oparin (1960) have found that calf thymus DNA inhibits the enzymatic activity of papain, but the specificity of such a reaction has not been investigated.

There are a number of studies on the effect of nucleic acids on the incorporation of radioactive tracers into various cellular fractions. Typical of these studies is that of Salganik (1958), who has studied the uptake of C^{14}-labeled glycine into the nucleoprotein fraction (determined by TCA-precipitation and hot TCA-extraction) of thymus

glands in which the DNA has been damaged either by treatment with enzyme or by X-rays. He finds that the incorporation rate decreases if the DNA has been affected, but that the incorporation rate can be raised to the original level by the addition of DNA from other animals. Perevoschikova and Zbarskiy (1957) have measured the incorporation of radioactive amino acids into the proteins of sarcoma slices as affected by hydrolysates of RNA. They find that RNA hydrolysate of yeast "stimulates incorporation into liver or spleen slices of labeled amino acids." The stimulation amounts to around 0.5 counts per minute per gram of protein; and since the original solutions contained 4×10^4 counts per minute, it should have been obvious that a check on physical absorption or other non-specific effects should have been made. None was made and the results may therefore be meaningless. If so, the authors' conclusion that "as long as DNA is preserved in the system employed, the products of RNA hydrolysis may serve for specific nucleoprotein synthesis" is completely unfounded. Toliušis (1958) has found that in a suspension of cytoplasmic granules of rat liver, plus the necessary additional substances, an increased turnover of RNA phosphorus occurs during rapid protein synthesis. The turnover seems to be, according to him, nonspecific.

The Synthesis of Nucleic Acids

There have been several papers purporting to show interconversion between RNA and DNA. Examples are those of Manoilov, Nemchinskaya, Alieva, and Mytareva (1957) and of Salganik, Morozova, and Kiknadze (1959). The experiments cited, however, are not carefully enough controlled, so that we cannot tell whether there really is any conversion from one to the other or just a breakdown by non-specific enzymes in the extracts that are used. Kritskiy (1957, 1958) has investigated the influence of DNA on P^{32} incorporation and the effects of orthophosphate and pyrophosphate on the decomposition of DNA, in pigeon liver homogenates. Experimental work of higher quality is that of Belousova (1955), who showed that RNA or its decomposition products increases the rate of incorporation of amino acids in homogenates and tissue slices.

In looking over this literature, the reader gets the impression that Soviet scientists have reported the occurrence of every possible type of interaction between DNA, RNA, and proteins. Just by chance, a correct result should sometimes be obtained.

Biologically Active Nucleic Acids

In work which seems comparable to that performed earlier in the West, Gavrilova and Spirin (1959) have investigated the relation between the infectivity of the RNA of tobacco mosaic virus and some of the physical properties of the RNA, such as optical density, viscosity, and sedimentation constant. They find that these physical properties do not change even when there has been a big loss in infectivity, and conclude that the infectivity loss therefore does not arise from a splitting of the RNA chain.

The Soviet literature indicates that there has been a change in opinion since the article in 1953 by Rashba in which he says that "the theory that DNA is a gene-bearing molecule is harmful and reactionary." Rashba felt at that time that "nucleic acid is the important nutritive substance under the influence of assimilation of which the metabolism of microbial cells is definitely modified." Recent reviews on the subject of transforming principle and general discussions of the role of nucleic acid have swung much closer to Western ideas upon these subjects, namely, that DNA and genes are very closely related if not identical. However, very little experimental work has been done in the Soviet Union on transforming principle, and the majority of articles are summaries of the foreign literature. Typical papers are by Loginova (1957), by Khesin (1958), who gives an excellent review of nucleic acids and genes, by Timakov and Skavronskaya (1958), and by Krivitskiy (1958) on nucleic acids and viruses. Krivitskiy's review, which is mostly concerned with work in the Western world, makes two points: first, that nucleic acids are very important elements in virus reproduction, as shown by the work on bacteriophages and tobacco mosaic virus; and second, that the participation of proteins is important in syntheses and the possibility of protein carrying some of the genetic information has not been ruled out. He cites Soviet electron micrographs as the first indication of the spiral structure of TMV and as showing that bacteriophages are composed of two components (protein and nucleic acid). He says that Gershenzon (1956c), in an article the reviewers have not seen, describes the separation of insect polyhedral virus into its

DNA and protein components, each of which alone is non-infective. When the virus is "reconstituted" from its components, it regains its infectivity. This report, if substantiated, represents the first separation and reconstitution of a DNA-containing virus. One of the few papers giving experimental results is that by Imshenetskiy (1959) on the transformation of streptomycin resistance from one bacterial strain to another.

Some of the other Soviet work on what seems to be, from their point of view, transformation, is almost impossible to evaluate because there does not seem to have been a careful control of purity of cultures, growth rates, etc. For example, Rashba and Galkina (1955) and Rashba and Kaganskaya (1957) report changes in the nucleic acid and nucleoprotein composition of *E. coli* after it has assimilated products derived from *S. breslau*. The new nucleic acid has a low viscosity and a different titration curve, but there is no control and no mention is made of the fact that the nucleic acid content of a bacterial system can change with the rate of growth of the culture and with the amount of nutrients in the medium.

Structure and Physical Chemistry of Nucleic Acids

Although earlier Soviet papers (see Rashba, 1953) deprecated the importance of DNA and its constancy of composition (Makarov, 1956), recent work recognizes the crucial importance of DNA in heredity. The importance of the helical structure of DNA is appreciated and actively used as the fundamental idea of DNA structure in theoretical discussions of replication, as for example, in the paper by Plishkin, Luchnik, and Taluts (1959), who point out that the two-stranded configuration of DNA is the minimum energy one and that a possible duplication scheme is for the new bases to be linked to the single strands of DNA before the inter-nucleic acid bonds are formed. They say:

> it has escaped the notice of all the authors of the various hypotheses that in at least six animal and plant species ring chromosomes are described which normally reproduce themselves. In a double ring helix neither slide nor uncoiling can lead to separation.

They are apparently unaware that others have considered this ring chromosome problem, and moreover they make no mention of the fact that chromosomes are not pure DNA, nor do they consider the possibility that the DNA chains may be at right angles to the chromosome axis rather than parallel to it.

A statistical theory of the duplication of DNA has been presented by Vol'kenshtein (1960). He has calculated the partition functions for the various combinations of nucleotides bonded to DNA and thereby computes the probability of a configuration in which the two original DNA chains will separate and subsequently become hydrogen-bonded to complementary bases in the surrounding medium. The details of the calculations indicate that a high molecular weight of DNA, i.e., a long chain length, is necessary if duplication by this mechanism is to be probable. He regards duplication as a thermodynamic transition.

Soviet physicists are becoming interested in the DNA coding problem. Chavchanidze (1958) has described a novel way of coding 20 amino acids by combinations of the 4 nucleic acid bases. He assumes that 3 bases specify an amino acid, but that some of the 64 possible combinations of bases (4 units taken 3 at a time) are redundant. He assumes that a sequence of 3 bases, say ATG, is equivalent to its complement, TAC, and that the direction in which the code is read is unimportant, so that other equivalent forms are GTA and CAT. Chavchanidze shows that there are 20 independent combinations of 3 bases. He has no commas in his code.

Diskina, Spitkovskiy, and Tongur (1958) have obtained experimental relations between the viscosity and the protein content of nucleoproteins. This subject is also discussed in a series of papers by Spitkovskiy and collaborators (1956, 1958). They find relations similar to those obtained by Doty and his coworkers in this country. They claim that DNA has a primary structure, namely, the order of the nucleotides in the chain, a secondary structure, the specific form of the double helix, and a tertiary structure, the way in which the double helix is bent and folded. In order to accommodate the many folds there must be a reasonably large number of breaks in the secondary structure. Brodskiy (1958) has examined by microspectrophotometry the relative RNA composition of different parts of the cells of paramecia, insect motor nerves, and retinal cells of frogs and mice. If the RNA composition is constant, the ratio of optical densities at two different wavelengths should not vary. This ratio does vary, and Brodskiy therefore concludes that the RNA content varies from place to place in such cells.

Belozerskiy and Spirin (1958) have obtained evidence that some of the RNA composition depends on DNA.

There have been reports in the Soviet literature of the antigenic properties of DNA preparations. Typical of these is that of Chamova (1958). He has isolated DNA from calf thymus and assayed its antigenic ability by complement fixation. He has evidence that the antigenic properties are dependent on the DNA and not on contaminating protein, because, he says, the antigenic ability is not dependent on the state of purification. It does depend on the average molecular weights, and treatment with deoxyribonuclease knocks out the antigenicity. In an earlier paper (Chamova, 1957) he remarks that trypsin does not affect the antigenic properties; but he also indicates that treatment of his preparations with trypsin does release some amino acids. None of these data is good enough to rule out the presence of protein carried along with the nucleic acid preparation.

Content and Development of Nucleic Acids

Excellent work on base ratios in DNA has been done by Belozerskiy and Spirin (1958), and by Belozerskiy, Shugaeva, and Spirin (1958). These workers found that in the DNA of actinomycetes the ratio G + C/A + T was greater than 2.5. This is the highest known ratio of all species. Within one genus they found no big species difference, the values of the ratio varying from 2.73 to 2.8. The big change in base ratios in DNA from one species to another is not matched by the changes in RNA content. They also found that the amount of RNA varies during the growth of cells. Such an anomalous behavior in the relations between DNA and RNA was also reported by Spirin, Belozerskiy, Kudlay, Skavronskaya, and Mitereva (1958), who pointed out that the changes in DNA content in the transformation of intestinal bacteria from one type to another were by a factor of 2, whereas the changes in G + C/A + U content in RNA changed very little. Vanjushin and Belozerskiy (1959a, b) have shown that the ratio G + C/A + T for maize and other plants goes through about the same range of values as for animals.

In studies which were somewhat similar to those performed by many workers, in which the nucleic acid content is correlated with the age of cultures, Evreinova, Bunina, and Kuznetsova (1959) measured the variation of nucleic acid content with temperature. As the temperature goes up, the nucleic acid content goes down and the mononucleotide composition of RNA changes. This may be a phenomenon which is similar to the changes produced by different rates of growth.

The changes in composition of nucleic acid polymers during the development of silkworms and during the germination of seeds have been studied by many workers. The effect of degradation products of DNA on tissue respiration and in oxidative phosphorylation has been studied by Chepinoga (1958), who obtained results similar to those found by Pinchot in this country. Roskin and Yulius (1955) investigated the RNA content of mouse mammary cancer cells. By starvation two extreme types of cells were obtained. Type A contained a large amount of RNA and type B contained little RNA and represented cells which were undergoing a negligible number of mitoses. One type became converted freely into the other, and the authors conclude "that a combination of properties called malignant is unrelated to the state of the RNA in the cytoplasm."

The partitioning of nucleic acids and nucleic acid bases among different phases in coacervates has been studied, for example, by Evreinova and Kuznetsova (1959), who find that "the larger the drop the lower the nucleic acid concentration. DNA gives higher concentrations in drops than RNA. The larger drops contain more total nucleic acid than do the smaller."

12. MUSCLE, NERVE, VISION, TASTE

There have been a few attempts to correlate the physicochemical properties of actomyosin with the behavior of muscle. Grishchenko (1955), of the Institute of Hygiene, Labor, and Occupational Health (Moscow), has made theoretical calculations of the work done from data on heat production, volumetric reduction and ATP utilization in actomyosin. The several methods of calculation give similar results. Assuming a gel swelling process, he arrives at the figure of 5 cal/100 g myosin. Kalamkarova (1958) has measured the birefringence of various proteins of the actomyosin complex as a function of velocity gradient, concentration, and index of refraction, and thus has been able to calculate intrinsic and form birefringence; but there is no analysis in terms of axial ratio or length. The myosin content is reported to decrease, and the amount of myogen to increase when cardiac muscle is subjected to anoxic conditions (Bondarenko, 1954). Kafiani (1953) has separated myosin

from actin, for the purpose of studying the contractile properties separately. His criterion for myosin purity is a lack of decrease in viscosity when ATP is added. There is maximum ATPase activity and best contraction of myosin at pH 9, although the corresponding pH for the actomyosin complex is 7. Kafiani has demonstrated the reversibility of myosin fiber contraction by contracting the fibers without a load in .05 M KCl, and then relaxing them in .25 M KCl *with* a load. Ivanov, Parshina, and Mirovich (1959), of the Biochemical Laboratory of the Institute of Obstetrics and Gynecology, Leningrad, have a different type of evidence that the contractile properties and ATPase activity of myosin are closely linked. At 4,000 atmospheres of pressure, myosin solutions have no ATPase activity and are unable to combine with actin. Likewise, a decrease in the viscosity of actomyosin gels is not observed upon the addition of ATP at this pressure. Kasavina and Kuneyeva (1950), of the Laboratory of the Biochemistry of Cancer (Academy of Medicine), point out the basic similarities in the actomyosin complex of a wide range of organisms (from man down to insects). Vorobiev (1957) claims that the combination of myosin and DNA is a contractile system, also requiring ATP for functioning; although without the presence of calcium ions, ATP causes lengthening. He gives no evidence as to the type of union between myosin and DNA. Ryzhkov and Loidina (1953) describe complexes of myosin and tobacco mosaic virus and of actin and TMV, both of which precipitate out of solution. The actin-TMV complex is broken down by ATP. There is no evidence to indicate the type of bonds involved here.

The effects of various physical and chemical external conditions on muscle activity have been the subject of numerous investigations. For example, Suzdal'skaya (1959) measured the excitability of pigeon muscle to long and short electric stimuli as a function of temperature. For stimuli applied for times like those of the physiological impulse, there is little effect of temperature (and thus a kind of built-in protection against temperature change), but effects of stimuli of longer duration are subject to variation with temperature. Shtrankfel'd (1958) used the damping of torsional oscillation, Q, as a function of temperature, to investigate viscous properties of various types of muscle. He points out that this property, Q, has a minimum when plotted against temperature, since there is a secondary rise in Q owing to predenaturation changes in the muscle protein. Kalamkarova and Muzheyev (1957) studied the flow birefringence, ATPase activity, and sulfhydryl content of myosin which had undergone various types of inactivation. They found that guanidine affects flow birefringence but has no effect on the —SH groups at the concentrations used; but sodium monoiodoacetate binds the —SH groups without altering ATPase activity. In general, when the enzyme activity is decreased, the flow birefringence (and thus the *shape* of the molecule) is altered. Sulfhydryl groups were shown by Koshtoyants and Bunkina (1959), of the Department of Animal Physiology, Moscow State University, to be important in the combined effect of potassium ions and the alkaloid veratrine—the effect changes if you block these groups with cadmium chloride. Measurements of the dielectric constant of muscle as a function of frequency have been made by Aladzhalova and Mertsalova (1958). In these experiments, which are similar to those of Schwann in the United States, it was found that the dielectric constant changes anomalously and that there is an absorption band at 200–2000 cycles per second. The frequency of the absorption band maximum is temperature-dependent. The authors conclude that muscle is a polar system with a "relaxation mechanism qualitatively described by Debye's general theory," and from measurement during and after stimulation that "no membrane mechanism seems to be involved in relaxation following slow contraction."

Koshtoyants (1958) believes he has evidence that DNA plays a role in the transmission of the nerve impulse. His evidence is very slight and dubious, and consists in the argument that since trypaflavine blocks vagus nerve action on the heart, and since adenine and trypaflavine together have no effect, and if it were known that DNA and trypaflavine form an insoluble complex under the conditions of this type of experiment, then we would conclude that DNA is involved. This paper of Koshtoyants is typical of many Russian papers on biological subjects: a minute amount of experimental data is accompanied by a relatively large amount of discussion and theorizing. Koshtoyants (1951, 1955) also is interested in the importance of sulfhydryl groups in excitation transfer. Kayushin and Lyudkovskaya (1955) have also investigated impulse propagation. They have developed a rather ingenious technique of observing with the interference microscope the motion of the surface of the frog's sciatic nerve as a shift of inter-

ference bands of light reflected from the nerve, when the impulse travels down it. However, they do not make clear how this type of data is of use in elucidating nerve mechanisms. Byzov (1957), of the Institute of Biological Physics, Moscow, describes a modified Swedish method for measuring action potentials from single retinal ganglion cells. He has also (Byzov, 1958) used a microelectrode fed by a pulse generator to determine the resistance and capacitance of the retinal layers in the frog. The values obtained are $C \sim 3\ \mu f/cm^2$ and $R_{max} \sim 300$ ohms/cm². Yakimchuk (1959) has tried to explain bioelectric potentials in terms of the potential differences (~10 millivolts) he finds between the pointed and flat ends of a conductor immersed in a conducting solution.

Yur'eva (1957) has an interesting experimental approach to the problem of taste. She uses action currents of a nerve in isolated frog tongues to measure reactions to various stimulators of the taste buds, and finds that the sensitivity decreases if the sulfhydryl groups are blocked with cadmium chloride.

Smirnov and Bongard (1959) have pointed out a possible analogy between retinal photoreceptors and semiconducting photoelements. The photoelements show transients when the wavelength is changed (even though the steady-state currents are the *same* at both wavelengths), because the various impurities have different wavelength sensitivities and time constants. The authors hypothesize that the retinal cones behave in a similar way. They make no mention of the similarity between their physical results and complementation in photosynthesis.

Glezer and Tsukkerman (1959) have analyzed the resolving power of the eye from the point of view of information theory and information storage. They consider storage of useful information in time and in space (i.e., on the retina) and derive relations relating the perceptible signal strength with the mean noise intensity and the duration and spatial extent of the incoming light.

SUMMARY AND CONCLUSIONS

Much of the description of technical instrumentation in the Soviet Union seems admirable. However, the application of instrumentation to biological problems has lagged behind that in the West, and many of the instruments and techniques have been copied from Western sources. Sometimes, as in the application of fluorescent techniques to the determination of bacterial concentration, the sophisticated instrumentation could just as well have been replaced by a much simpler one that would possibly have been more accurate. Part of the difficulties along these lines arise because of the relatively poor state of biology in the U.S.S.R. as compared to physics, and the consequent tendency for biologists to use advanced physical techniques with the hope that this will further biology. Unfortunately, many of the biological concepts which are held are too primitive for biology to profit in this way.

Many of the papers in the field of the effects of ionizing radiation consist only of descriptions of effects, with few attempts to elucidate mechanisms. The quality of the instrumentation seems far higher than that of the biological techniques used. There is more interest in the use of ionizing radiation in vaccine production and antitoxin sterilization than in the West, yet so far apparently without notable practical success. The published work on the effects of ionizing radiation on macromolecules shows a large variation in quality, ranging from that of investigators such as Oppel, who are competent in protein chemistry, to that of those who are not.

Four generalizations may be made about the flood of literature on the effects of ionizing radiations on the cell: (1) many workers fail to use a wide range of doses, and may even draw conclusions from results obtained with a single dose; (2) there is almost no interest in linear energy transfer; (3) some of the work on radiation modifiers is careful, and there are indications that at least some investigators are aware of the important foreign papers, but most of the work seems unsophisticated; and (4) there are many verbose theoretical papers which contain little or no evidence for the theories advocated.

Compared to the work on the effects of ionizing radiation, there is little work on the destructive effects of light on biological systems. Most of it is concerned with the production of improved antibiotic mutants and vaccines, without great success, not only because of the inherent difficulties in such experiments, but also because of confused thinking. The best work is the discovery of a relation between the thermal and ultraviolet sensitivity of proteins and work in the field of energy transfer as measured by fluorescent spectra of amino acids, proteins, and nucleoproteins. Experimental work of dubious value continues to be done in the field of mitogenetic radiation.

There is no lack of knowledge of foreign work

on ultraviolet effects, as shown in the review by Alikhanian and Mindlin (1956). This review, which is comprehensive and excellent, is concerned with the Western literature, an indication that little work of this type up until 1956 was carried on in the Soviet Union. That part of the later work which is closely connected with physics or has been done by physically trained personnel is excellent. A higher proportion of the papers report incident light intensities than do those published in the West. The greatest scientific failure seems to be the lack of communication between the physically and biologically trained workers, or the poor biophysical training of the persons attempting to do these experiments, as, for example, in the complete disregard of the lack of transparency of irradiated samples. As work in the microbial and biochemical fields improves in quality, so will the investigation of the action of light on biological systems. Because of the more physical interest in this field, it may represent the starting-place for a sophisticated attack on problems of microorganisms and biochemistry.

Soviet work on photosynthesis, especially its physical aspects, is characterized by a high level of competence. The strongest areas are associated with measurements of fluorescence and its polarization and lifetimes, energy transfer among pigment molecules, charge transfer, and the mechanisms of the initial acts involved in photosynthesis. A number of Soviet workers believe that the semiconductor model is an oversimplification, and that radical ions, or triplet states, plus intermediate enzyme and redox systems, are the intermediates between light absorption and photosynthesis. Work of a high level of excellence continues on the photoreduction of DPN in chlorophyll solutions and on the spectrochemical changes associated with labile pigment forms with free radical properties. The purely biochemical and biological investigations in this field are not of such high caliber.

Ultrasonic research is more extensive than in the West. Most of it is associated with the name El'piner, who has published extensively and repetitiously. Experiments have been performed on the effects of ultrasonic waves on molecules and microorganisms, and on the ability of ultrasonics to break up cell structures and release enzymes or toxins. The physical instrumentation is usually well described, and the physical measurements are carefully done. Many of the effects of ultrasonic vibrations on cells and cell components are similar to those obtained with ionizing radiation, and some of the explanations given for the effects are ascribed to the free radicals produced by ultrasonic vibration. One gets the impression that El'piner and his group are well in control of this physical technique, and are using it for every conceivable problem. Because ultrasonic waves are nonspecific in action, the amount of useful biological information that has emerged from all of this work is small.

Soviet work in the field of paramagnetic resonance, mostly carried on by Blyumenfel'd and Kalmanson, is of superior quality. One may note that this is a field closely allied to physics. They have made quantitative investigations (which is more than has been done in much of the Western work) on the effects of radiation in producing electron spin resonance in proteins and amino acids, and have arrived at the conclusion that in normal proteins, which have not been denatured by heat, there may be migration of electrons through the structure of the protein. The general technique has been applied by the above workers to investigation of nucleic acids and of mechanisms of enzyme reactions.

Biophysical studies of microorganisms are hampered by lingering Lysenkoism, in the form of many studies of "directed variability." The biological side of these investigations is often extremely naive. However, some work, such as that of Spirin and his collaborators, shows a really molecular approach to the bacterial cell. The same generalizations may be made about the biophysical studies of viruses. Some of the electron microscopy suffers from a lack of attention to important biological details. There are no X-ray diffraction studies of virus structure.

Dialectical materialism also invades the study of cell structure, although many workers have another approach to the subject. Authors in the latter category are well acquainted with Western work. The use of physical techniques in this field is markedly lacking, except for a few scattered papers on electrical measurements.

There is more loose thinking in the field of protein study than in any others except microbiology and virology. Some workers are surprised that casein is not a pure protein, and there is a lack of appreciation that proteins have primary, secondary, and tertiary levels of structure. Little concrete work is being done on the mechanisms of enzyme action or on the use of enzymes as specific reagents. The Soviet literature on proteins, with

the exceptions mentioned, shows an almost complete absence of the use of physical techniques, such as sedimentation, diffusion flow birefringence, and viscosity measurements. Excellent work has been done on the structure, properties, and synthesis of collagen, procollagen, and pepsin, and on the amino acid sequences in hemoglobin. Interest in the mechanism of protein synthesis and the specification of amino acid order has stimulated experiments on the incorporation of amino acids into specific proteins. These experiments indicate that proteins are made directly from amino acids and that there is negligible renewal of amino acids in a protein already formed. Experiments which indicate that small fragments of trypsin and aldolase have enzymatic activity and that tryptic digests of serum albumin are reconstituted at high pressure should be repeated in Western countries.

The review articles on nucleic acids written in the Soviet Union show that many Soviet scientists are familiar with Western concepts and Western techniques. In the field of nucleic acid study most of the ideas and techniques, with the exception perhaps of chromatography, are derived from the Western literature. The amount of good fundamental research, however, is small, with the exception of work on base ratios in various systems, as in the papers of Belozerskiy and his coworkers. The emphasis in the review articles indicates that more and more attention is being paid to the relations between the biological activities of such nucleic acids as transforming principle, and their physical properties.

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DOES THE MELANIN PIGMENT OF HUMAN SKIN HAVE ADAPTIVE VALUE?

An Essay in Human Ecology and the Evolution of Race

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ABSTRACT

The widely accepted idea that melanin pigment in human skin protects against sunlight, and that this has bearing upon adaptation to life in the tropics and the distribution of races, is examined in terms of its physical and physiological aspects. Regarded in such terms the concept appears to have little merit. It is concluded that whereas the pigment may have a slight adaptive value as regards some aspects of the organism-environment relationship, it may be non-adaptive as regards others; and the respective values may depend upon various complicating factors of the environment. Moreover, the distribution of races according to skin color does not appear to conform well to what would be expected from the spectral distribution of sunlight.

IT is unnecessary to point out my admiration in general for the great modern synthesis which the science of evolution represents, but I may venture to observe that some examples of adaptive mechanisms are being paraded where the physiological interpretations are taken much too lightly."—P. F. Scholander (1956).

Melanin pigment plays a predominant role in giving the brown and black color to the skin, which provides some of the most striking differences in outward appearance of human races. This would seem a very superficial difference, yet there has been attempt to find adaptive significance in the presence of the pigment itself. It is quite generally accepted, without qualification, that the pigment constitutes a protective barrier against injurious effects of sunlight. Reasoning on this basis, Negroes have been thought to be better adapted than white-skinned peoples to life in the tropics, and this has been attributed to natural selection. Charles Darwin himself made this suggestion, although he hedged it with characteristic caution. The argument has been expanded since, and the idea has come to be widely accepted. This is sometimes cited today as a definite example of natural selection in Man.

In Darwin's day very little was known about the effects of sunlight on the human body or their relation to the spectrum of sunlight, nor about the geographical distribution of sunlight; and although a good deal has been learned since, the knowledge does not seem to have been widely disseminated. There are still many things to be explained, but enough is known for a reasonably critical examination of various aspects of the above thesis. When one attempts such an examination he cannot but be surprised that such far-reaching conclusions, based on such tenuous evidence, should have received so much credence.

MELANIN AND ITS POSITION IN HUMAN SKIN

The term melanin is applied to a type of finely granular, dark-colored substances in skin, hair, and some other organs of animals. While there is basic similarity in chemical composition there is a good deal of variety among the types of melanins. The color, which ranges through various shades of brown to black, is strongly affected by scattering of light by the pigment particles. The latter factor makes it difficult to relate the color to chemical composition, because of the difficulty it introduces into the measurement of absorption spectra.

In human skin the melanin is normally contained in the *epidermis*—a thin outer layer ranging from about 0.07 to 1.2 millimeters in thickness.

The relationship of the epidermis to other structures, and some approximate dimensions, are indicated in Fig. 1, B. In discussing the optical properties of skin, which concerns us here, it may be necessary to oversimplify the details of structure, which are quite complex. To this end I shall treat the epidermis as composed of two layers; the *malpighian*, made of living cells; and exterior to this the *corneum*, a dead layer which is produced from the living malpighian. The optical properties of these two layers differ greatly, although it would be difficult to demonstrate a sharp boundary between them, either optically or biologically. The distinction is useful, however, because the corneum obviously serves as an efficient barrier to ultraviolet light, greatly diminishing the amount that reaches the viable cells of the malpighian. Beneath the epidermis is the dermis, where are found a variety of structures, including the hair follicles, sebaceous glands, and sweat glands. The most superficial blood vessels are located in the dermis just underneath the epidermis. The melanin is produced in specialized cells, the *melanocytes*; but passes into neighboring cells of the malphigian, and may be carried up into the corneum. For a discussion of the melanocytes and the general histology of the epidermis the reader is referred to the excellent review of Billingham and Silvers (1960). In the white races the corneum and upper part of the malpighian may be relatively free of melanin except after injury—including the action of ultraviolet light which results in the familiar suntan. Negro skin, on the other hand, not only contains more melanin, but this is more evenly distributed throughout the epidermis, including the corneum.

The distinction made here between "negro skin" and "white skin" is perforce somewhat arbitrary. With few exceptions neither optical nor histological studies specify exactly the race or origin of the subject; and one can only assume that these terms apply to some reasonably well distinguished extremes of pigmentation. There seems to have been little or no optical study of skins of intermediate color, beyond reflectance measurements, which are not very instructive in the present regard.

While the study of evolution of races is one of separation of genotypes, the role of natural selection in such a process must depend upon interrelationships between phenotype and environment, and it is at this level that we find ourselves in the present discussion. Thus the mechanism of inheritance of the melanin of human skin need not enter directly here. This is fortunate because of the complicated nature of the problem, into which many facets of the phenotype may enter. For example, a matter of first importance is the penetration of light into the skin, and while one may measure this directly with some success the contributing factors might be very difficult to analyze. They include the absorption and light scattering properties of the various layers; and to what extent melanin is concerned in this must depend upon such factors as dispersion and distribution of the pigment, its chemical composition and spectral absorption, and the character of the other components. To disentangle the genetics pertinent to all these factors, some of which we do not understand at all clearly, would be a difficult matter indeed. For the recent status of genetic studies on melanin of mammal skin, the reader is again referred to Billingham and Silver (1960).

SUNLIGHT AND ITS PENETRATION INTO SKIN

Fig. 1, A shows the spectral distribution of sunlight at the surface of the earth, when the sun is at two different angles; curve 1 for the sun at zenith, and curve 2, for the sun at 60° (four hours) from zenith. The lower wavelength limit (i.e., in the ultraviolet) is set at about 0.29 μ by absorption of the shorter wavelengths by ozone in the upper atmosphere. The serrated appearance in the infrared part of the spectrum is due principally to spectral absorption bands of water vapor. The human eye detects only a small part of this spectrum, as the line labelled V indicates—normally a range from about 0.4 μ to 0.65 μ. Thus the eye is a poor judge of total sunlight, and no judge at all of the ultraviolet or infrared parts. The very short wavelengths of sunlight are those which cause sunburn, that is, a range from the lower limit at 0.29 μ to about 0.32 μ, as indicated at E in the figure. This is a very tiny fraction of the total (less than 0.1%) under the maximal conditions represented by curve 1; and is reduced to a negligible amount when the sun moves a few hours from zenith, as indicated by curve 2. On the other hand, total sunlight varies much less with zenith angle, that is, with season, latitude, and time of day, as is easily seen by comparing curves 1 and 2 for the whole range of wavelengths.

In order to interpret the various physiological effects of sunlight on Man, it is necessary to have some idea of the extent of penetration of the different wavelengths into the skin. But this is not an easy matter to determine, and although a number of dependable measurements have been made for

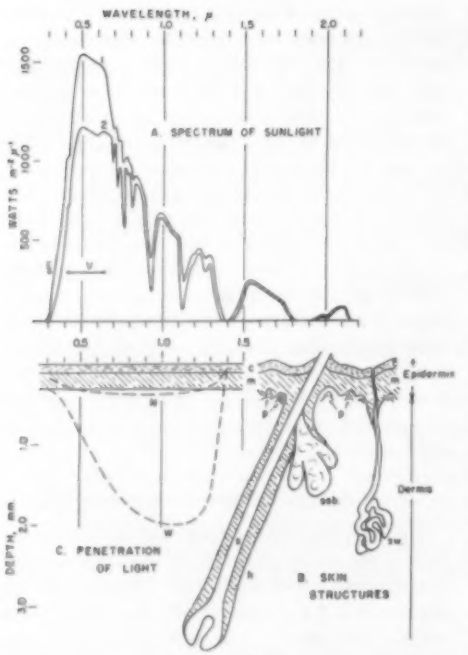

FIG. 1

A. SPECTRAL DISTRIBUTION OF SUNLIGHT AT THE SURFACE OF THE EARTH (after Moon, 1941). Curve 1, with sun at zenith. Curve 2, with sun at 60° (four hours) from zenith. V, spectral limits of human vision. E, spectral limits (within sunlight) for sunburn, antirachitic action and cancer induction.

B. DIAGRAMMATIC REPRESENTATION OF SKIN STRUCTURES. A schematized conception in which the dimensions should not be taken as generally representative, since the skin may vary widely in its thickness. c, corneum, i.e. horny layer of epidermis. m, malpighian layer of epidermis. sw., sweat gland. seb., sebaceous gland. p, the most superficial blood vessels, arterioles, capillaries, and venules. h, hair follicle. s, hair shaft.

C. PENETRATION OF LIGHT INTO HUMAN SKIN AS A FUNCTION OF WAVELENGTH. The curves N and W indicate for Negro and White skin, respectively, *rough estimates* of depths at which radiation of the corresponding wavelengths is reduced to 5 per cent of its incident value. There are insufficient data to make more than rough estimates, and these curves should be regarded as suggestive rather than in any way exact (see text). Curve W is based on Hardy and Muschenheim (1934, 1936), and on Kirby-Smith, Blum, and Grady (1942). Curve N is based on Hardy and Muschenheim (1934, 1936) and on Thompson (1955).

limited spectral regions these cannot be put together into more than a rough composite picture. Skin is made up of optically inhomogeneous layers, having different properties and varying in thickness and structure from one part of the body to another, and since different investigators have measured different samples of skin the results cannot be fitted together accurately. Even with more complete measurements no universal values for skin transmission could be assigned which would apply to skin from all parts of the body. The curves drawn in Fig. 1,C, which purport to show the depth of penetration of 5 per cent of the incident radiation, for negro skin (N) and for white skin (W), cannot therefore be accepted as more than rough estimates; but I think they will be useful and not misleading if they are employed only as approximate guides.

Referring to these curves, it is seen that the sunburning radiation (wavelengths shorter than 0.32 μ) is nearly all absorbed in the epidermis—it is here, then, that this radiation must have its effect. Penetration increases toward longer wavelengths—reaching the superficial blood vessels and other structures of the epidermis—until a maximum depth is reached around 1.0 μ in the infrared. The penetration then decreases rapidly as wavelength increases, there being little of importance beyond 1.4 μ, within the spectral range of sunlight. While some investigators have claimed greater transmission of some wavelengths in the visible and infrared, it seems clear that no portion of the radiation of sunlight penetrates in important amount below a few millimeters. General discussions often fail to recognize the great importance of differences in penetration of different spectral regions of sunlight, and that different regions have different physiological effects (e.g., Coon, Garn, and Birdsell, 1950).

Faulty measurements have led from time to time to misconceptions regarding the depth of penetration of sunlight into skin. Important errors may enter in two ways: (1) low measurements are obtained when scattering is neglected, this error being increased by drying out of the skin sample; (2) if care is not taken to minimize heating of the skin by absorbed radiation, or proper shielding is not provided, the measurements may indicate too great penetration. In preparing the curves in Fig. 1, C, the measurements of Hardy and Muschenheim (1934, 1936), of Kirby-Smith, Blum, and Grady (1942), and of Thomson (1955) have been relied upon. For a discussion and other references, see Blum (1945a).

The curves in Fig. 1, C neglect the specific absorption bands of hemoglobin, carotenoids, and other substances. Hemoglobin in the superficial vessels absorbs the shorter wavelengths of the visible spectrum quite strongly, so that light reflected back in predominantly red, this giving the

ruddy tint to many complexions. The curves indicate that negro skin is less transparent in the visible spectrum than white skin, and this is no doubt due chiefly to the difference in melanin content. But negro skin also has a thicker corneum than white skin, an important factor with regard to sunburning radiation (see below).

PHYSIOLOGICAL EFFECTS OF SUNLIGHT ON MAN, AND FACTORS AFFECTING THEM

Sunlight has several physiological effects on Man, which may be separated into two general categories on the basis of the spectra involved: (1) the effect of adding the energy of sunlight to the heat load of the body; and (2) specific effects of those wavelengths that cause sunburn, which also have carcinogenic and antirachitic action. The latter are classed together, not because of physiological similarity, but because they are produced by the same spectral region of sunlight, since this factor is of basic importance if we are to relate them to geographical distribution.

At least one other, minor, effect will be mentioned below, and there are certain pathological conditions brought about by parts of the solar spectrum (e.g. see Blum 1941, 1950) which are so rare that they are of no interest in the present discussion. There are also diseases of domestic animals in which sunlight is the precipitating factor; but which, for physiological or dietetic reasons do not have their counterpart in Man (e.g., Blum, 1941; Clare, 1955).

THE SOLAR HEAT LOAD

In the standard resting state the human body produces a certain amount of heat by its own metabolism, which must be dissipated in some way. If the body becomes active this heat production increases, and in case of severe exercise may rise several fold above the resting condition. The human body has several means of getting rid of this heat load. The blood, heated internally, comes to the surface to pass through the vessels of the skin, and a certain amount of heat may be *conducted* to the surrounding air. The rate of dissipation is increased by movement of air over the skin, that is, *convection* is a factor. Another important means of dissipating heat is by *evaporation* of water from sweat at the surface of the skin. A factor less commonly recognized and more difficult to assess is the loss to cooler surroundings by *radiation*.

All bodies emit radiation according to their temperature and size. In the ordinary range of temperatures, all this radiation is in the infrared spectrum and hence invisible, although it may be detected through the sense organs of the skin as a sensation of coolness, warmth or, if intense, of pain (Oppel and Hardy, 1937 a, b; Hardy and Oppel, 1937). The human body radiates to its surroundings and receives radiation from them; it may gain or lose in the exchange, for example, it gains heat from a hot radiator or loses heat to a block of ice, even though these objects are at some distance. But there is also a constant imperceptible or barely perceptible exchange with objects nearer the temperature of the body—indoors with the walls of the room, outdoors with hot soil or rocks or the cool foliage of plants. It is clear that the effective temperature of the environment, with respect to the human body, is a complex matter, and is not to be measured adequately in terms of the temperature and humidity of the ambient air alone.

Out-of-doors one may receive an important amount of heat from direct exposure to the sun; that is, the total spectrum of sunlight impinging upon the body adds a certain amount of heat load. On the other hand, the body may lose a considerable amount of heat by radiation to the clear sky, which is, effectively, at a lower temperature than the body, the amount of loss through this channel being largely determined by the quantity of water vapor in the atmosphere. This radiation to "space" may be an important factor in desert areas, where the atmospheric water vapor is low (see Blum, 1945b, for a consideration of various factors involved under desert conditions).

It is clear that the human body is constantly exchanging heat with its environment in a number of ways. Hence the amount of heat lost, and the means of losing it, must vary according to the particular environmental conditions, and some of these may be difficult to evaluate. The radiation factor is among the most difficult to assess because it involves both the geometry of the human body and that of the surroundings, since the amount of heat exchange by radiation must depend upon the profiles which the radiating masses present to each other (see Blum, 1945b). The heat dissipation is modified by clothing, which may prevent the conduction of heat away, and may absorb or reflect the incident radiation according to the wavelengths concerned. Thus, dissipation of the heat load—which is essential if the man is going to live—becomes quite a complicated matter, and one that can be treated only with reference to the particular complex of environmental conditions that may obtain.

If the conditions are such that the man cannot adequately dispose of his heat load the temperature of the body must rise, but there are no climatic conditions in the world inhabited by Man where death is likely to result directly from raising the body temperature—the body is not likely to be "cooked" by coagulation of its proteins. Even under maximum conditions, the energy of sunlight is not nearly sufficient to burn the skin by heating unless concentrated by means of a lens, sunburn being caused by ultraviolet light acting in a very different way. Contrary to common impression, there is no evidence that the brain may be unduly heated by exposure of the head to direct sunlight (see Aron, 1911).

A man may, however, suffer collapse due to circulatory failure under environmental conditions that lead to excessive water loss, as, for example, in desert climate, and sunlight may be a factor in this. If the intake of water is not sufficient to compensate for the loss by evaporation and through other channels, the blood volume may fall so low that the heart can no longer maintain sufficient blood flow. The resulting collapse may be sudden, although the condition of low blood volume has accumulated for hours. Being usually associated with high environmental temperature, such collapse has been called "heat stroke," or—since it often occurs when the person is exposed to the sun—"sunstroke." The factors contributing to the depletion of water may be complex. They are most easily analyzed, perhaps, in the case of desert conditions where the temperature of the air is above that of the body, so that loss of heat by conduction and convection is virtually nil, and cooling must be accomplished almost entirely by evaporation of water from sweat. Such conditions may be exaggerated by the solar heat load, coming both directly from sunlight impinging on the body and also from reflection and reradiation from heated surroundings. Walking under these conditions, a man may lose as much as one liter of water per hour, and if this is not adequately replaced the blood volume must be eventually reduced, with ultimate collapse (Adolph et al., 1947).

TABLE 1

Estimated relative absorption of sunlight by White and Negro skin, based on Heer (1952); values for color temperature 6000°C, which is approximately that of the photosphere of the sun.

WHITE		NEGRO	
untanned (inner forearm)	tanned (forehead)	untanned (inner forearm)	tanned (forehead)
1.00	1.16	1.36	1.46

Under such conditions the radiant energy absorbed from sunlight may constitute a critical increment of heat load, and the man who can most readily reduce this increment would have an advantage, and might survive longest. In this case the man whose skin reflected more of this load should be better off. Table 1 indicates that negro skin absorbs roughly 30 per cent more sunlight than white skin; but it is to be remembered that this figure applies to only a part of the total heat load. Thus, if a Negro and a white-skinned man walked side by side, naked, in direct sunlight, across a desert where the temperature of the air was above that of the body, the Negro might be expected—other things being equal—to collapse before the white man. But these particular competitive conditions are not likely to be often met. If the bodies of both men were similarly clothed or otherwise covered any advantage would, of course, be largely lost.

It has been suggested that the melanin of the Negro, because it brings about greater local heating of the skin, leads to more profuse sweating, and that this is of advantage to him in the tropics Thomson (1951) has recently shown that sunburn tends to reduce sweating in white skins to a greater degree than in negro skins, and this might conceivably give the Negro a certain advantage in this regard. But while profuse sweating may confer a somewhat greater degree of comfort under conditions where water is rapidly evaporated, it could only increase the water loss, and this would seem to be the factor of real importance—a disadvantageous one—with regard to survival.

Taking all things into consideration, it seems necessary to assume that the possession of a dark skin should be a disadvantage to the Negro, as regards heat load and life in hot desert areas, but that the disadvantage is not a very great one, and probably of little importance under his usual conditions of life. It would seem that, if anything, his melanin pigment might be of some, although limited, advantage to the Negro in a cooler climate where it was important to conserve rather than to lose heat.

EFFECTS OF ULTRAVIOLET LIGHT

We now come to consider some other effects of sunlight in which melanin may also play a role. These are: (1) *sunburn*, a phenomenon with which

everyone is acquainted; (2) *carcinogenesis*, i.e., the induction of cancer of the skin; and (3) *antirachitic action*, the prevention of rickets, a disease of bone. All three effects are caused only by wavelengths shorter than about 0.32 μ, which, as we have seen, constitutes a very tiny fraction of sunlight. The fact that all three effects have the same long wavelength limit does not mean, however, that all are closely related. The first and last are very separate entities; the second is probably related to the first but this cannot be said with complete assurance (see Blum, 1959).

Sunburn

To most readers sunburn means an unpleasant blistering of the skin following exposure to sunlight. I use the term, however, to include a complex of related effects, ranging from a mild reddening of the skin, or *erythema*, to the severe blistering just mentioned. As a result of any degree of sunburn, melanin may increase in the epidermis—this we recognize as *suntan*. (For a more complete discussion, of sunburn, with extensive references see Blum, 1955a).

The erythema is the manifestation of dilation and consequent increased blood content of the minute vessels lying just beneath the epidermis. The photochemical reaction bringing about the dilation of the vessels has its locus in the epidermis, however, principally in the viable malpighian, where histological examination shows that cells may be injured or killed. The dilation of the vessels in the dermis is presumably due to substances which diffuse down from the injured epidermis. The corneum or outer horny layer of the epidermis protects against sunburn by absorbing a large proportion of the incident sunburning radiation before it reaches the malpighian—it is a very effective absorber.

While it seems safe for our purposes to consider the malpighian to be the locus of the principle changes underlying sunburn, the overlying corneum serving only a protective function, certain factors should be mentioned in this regard. Studies by Rottier and Mullink (1952) and Rottier (1953) indicate that photochemical changes brought about in the corneum may contribute to the erythema. But the eliciting wavelengths are, at least for the greater part, shorter than those found in sunlight, so this finding can have little or no concern in the present context. The sunburning radiation is absorbed in greatest part in the epidermis; but, at least in some white skins that have not been exposed for some time the longer wavelengths may penetrate to a small extent below, particularly in unexposed areas. The small fraction which reaches the minute vessels of the dermis may cause damage there, as shown by the inhibition of erythema (Blum and Terus, 1946); and it is possible that this may contribute to the blistering that occurs in some cases after severe exposure to sunlight.

The brownish color, or *suntan*, which develops some days after exposure of white skins to sunlight, involves the production of new melanin in the epidermis, although the earliest stage may represent migration of melanin that is already present, to a more superficial position. The formation of new melanin seems to be a response to injury—it also follows other kinds of insult to the epidermis. The color begins to fade after a time, but traces may persist for years. The fading is not all the result of removal or destruction of the melanin, but represents in part bleaching of the pigment by reduction, and the pigment may be subsequently darkened again by oxidation. It was shown just before the second World War by Henschke and Schultze (1939 a, b), that the bleached pigment may be caused to darken by exposure to longer wavelengths than those that elicit its formation. These wavelengths—0.3 μ to 0.44μ —are much more plentiful in sunlight, and much less affected by the angle of the sun, than are those that produce the suntan. Darkening of preformed pigment may also be brought about by endocrine secretions, and probably by other influences mediated by the circulation. The pigment-darkening reaction seems to have no physiological significance in itself; but it is interesting to us, because it shows that the color of the skin cannot be taken as a reliable quantitative index of the amount of melanin present in the human epidermis. The degree of bleaching and darkening seems to vary widely in white skins.

It is common knowledge that the development of suntan is generally accompanied by a decreased sensitivity to sunburn, and it is perhaps natural to relate the two events—the eye tells us that the skin has darkened, and we may jump to the conclusion that it transmits less light of all wavelengths, including those we do not detect. About 1899, Niels Finsen performed a simple experiment which he supposed to prove this point. He painted areas of his arm with India ink before exposing it to sunlight. When he scaled the ink off later he found that the areas it had covered had not been sunburned. He reasoned that the melanin in the

skin has the same protective effect as the India ink. Very little being known about sunburn at the time, he did not recognize that while India ink is opaque to ultraviolet light, he had no evidence that the same was true for melanin. Nevertheless Finsen's interpretation was accepted for 20 years without question, and still is by the majority of people.

About 1920, however, doubts began to arise as the result of observations by several investigators. It was noted that the immunity to sunburn disappears much more rapidly than the suntan—the immunity is usually gone in two months, whereas the tan may persist for years. And it was found that vitiliginous and albino skins, which do not form melanin, may develop a degree of immunity to ultraviolet light. An explanation was given in 1926 by Guillaume, who found that after exposure to ultraviolet light the epidermis, particularly the corneum, becomes thickened, and attributed the accompanying immunity to the enhanced absorption of the sunburning radiation in the latter layer. His findings were soon confirmed, and somewhat later it was shown by direct measurement that albino mouse epidermis increases its absorption of ultraviolet light after repeated exposures (Kirby-Smith et al., 1942). As a result, it was generally accepted by those actively working in this field that the relative immunity to sunburn which follows exposure is due to a resultant thickening of the outer horny layer, although the acceptance of the role of melanin pigment in this regard persisted, generally, outside this small group. After all, there seemed no good reason to think that melanin should be a better absorber of ultraviolet light than the protein which makes up the greater part of the corneum.

But Negro skin is very refractory to sunburn, and if this could not be accounted for by high melanin content, some other explanation was needed. The epidermis of Negro skin had been reported to be thicker than that of white skins, and it seems to have been generally assumed by those working in this field that it was the thicker corneum which made the Negro skin less susceptible to sunburn—at least I accepted this explanation myself. A few years ago, however, Thomson (1955) made a comparative study and found that Negro corneum was indeed thicker than that of white skins, but also that the former was more opaque to the sunburning radiation per unit thickness. It seems reasonable to attribute the greater opacity to the higher melanin content, although this may not be fully established. The corneum is composed principally of protein which absorbs the sunburning radiation strongly; but its effectiveness as a protective light-filter is greatly enhanced by its flake-like structure, which scatters the light and thus increases the path-length through which the light rays must travel in the absorbing medium. The amount of melanin is much less than that of the protein, and it is unlikely that it is a much better absorber of the sunburning radiation; but because it is made up of small particles it is an effective scattering agent, and this may make it an important additional factor in the absorption of the sunburning radiation by the corneum. Thus, after all, the melanin may play a considerable protective role, as regards sunburn—but all we know definitely is that the horny layer of the epidermis of Negro skin is a more effective shield against the sunburning radiation than is the epidermis of white skins. Thus, while it is reasonable to assume that melanin plays a role in protection against sunburn—and this assumption will be made in the following discussion in this paper—it should be kept in mind that the point is not definitely proven.

But can immunity to sunburn be regarded as having survival value? Sunburn is essentially an acute effect, which while it may be briefly debilitating in severe cases involving a considerable area of the body, does not cause prolonged, systemic damage. It is seldom disfiguring, although it may cause deterioration of complexion after many years of exposure—this coming late in the lifespan is not likely to play a role in sexual selection. In many white societies suntan is looked upon today as a sign of health and beauty. As regards the female sex, fashion reversed itself in this regard about half a century ago, but the survival of the societies concerned does not seem to have been seriously affected thereby.

Cancer of the Skin

Although it does not seem to be a matter of common knowledge, clinical, experimental, and geographical evidence converge to indict the sunburning portion of sunlight as a principal cause of cancer of human skin (see Blum, 1959). The cancers involved grow from the malpighian layer of the epidermis—many of them are not very malignant and mortality from this cause is relatively low. In white skinned people these cancers are limited almost exclusively to the exposed parts, particularly the face. They are very rare in Negro

skin, and when they occur show no preference for the exposed parts. There seems every reason to draw a parallel with the Negro's immunity to sunburn, attributing his low incidence to skin cancer likewise to the opacity of his corneum. Negroes do not show comparable immunity to other types of cancer, and indeed it is reported from the Kenya region of Africa that cancers of the skin occur not infrequently among natives in association with parasitic infection (Vindt, 1935).

Skin cancer does not, so far as we know, result from a single bout of sunburn, no matter how severe, but is a cumulative, "chronic" effect of exposures to sunlight repeated over a long period (Blum, 1959). Since these cancers are usually not lethal, and not very disfiguring except in late stages—as a rule they do not appear until late in life, well after mating—it would seem unlikely that cancer due to sunlight could have any great effect on racial survival in the sense of biological evolution.

Clear distinction should be made between these cancers and the very dangerous, though fortunately very rare, malignant melanomas which are cancers of a different type. The latter occur largely on parts of the body not exposed to sunlight,

Antirachitic Action

The same wavelengths that cause sunburn bring about an entirely different photochemical reaction—transformation of precursor substance into vitamin D. The site of the reaction can hardly be deeper than the epidermis, where these wavelengths are principally absorbed. It is difficult to estimate the effect of melanin on the efficiency of sunlight in producing Vitamin D—it might decrease formation by absorbing some of the effective radiation, or it is conceivable that by scattering in the corneum and consequent increase in path length, it might even enhance vitamin production in that layer.

Vitamin D is required for bone development and if it is lacking, the bone disease, rickets, results. Exposure to sunlight may prevent or cure this disease; but Man is not strictly dependent on sunlight in this regard, since vitamin D may also be introduced in the diet, and under most conditions of life the role of sunlight is probably not very great. Rickets is most frequent under crowded urban conditions where both diet and sunlight are limited. The disease most often affects children, and, since it is of crippling nature, its prevention could have survival value. But because of the complicating factor of diet, it would be difficult to estimate just how important sunlight might be as regards a given population living under natural conditions.

A TENTATIVE BALANCE SHEET

It is clear that the part played by melanin in the physiological responses of the human body to sunlight has aspects which require analysis in different ways, with regard not only to sunlight but to other environmental factors as well. Any classification of these aspects with regard to their advantages or disadvantages must be a very rough one. Still, it may be worth while to draw up a tentative balance sheet at this point in our discussion. In the one that follows, plus or minus ratings indicate estimated relative advantage of the high melanin content of negro skin as compared to the low melanin content of white skin.

PHYSIOLOGICAL EFFECT	NEGRO SKIN	WHITE SKIN
Solar heat load at high environmental temperature	−	+
Sunburn	+	−
Cancer of the skin	+	−
Prevention of rickets	±	±

Presented in this way the result seems equivocal, Negro skin having an advantage as regards sunburn and skin cancer, but a disadvantage as regards heat load at high ambient temperature. The advantage or disadvantage as regards antirachitic action is uncertain, but the latter seems the more probable. The balance sheet avoids the question of whether the advantages listed have significant survival value or not, and also how they might be related to the distribution of sunlight with latitude, that is, between the tropic zone and other zones. The latter question must now be taken up.

DISTRIBUTION OF SUNLIGHT WITH LATITUDE AS REGARDS PHYSIOLOGICAL RESPONSES OF MAN

On the whole, those who have assumed that Negroes inhabit the tropics because their skin pigment adapts them to the conditions of insolation in that zone, seem to have given little attention to the spectral character of sunlight and its distribution with latitude. It seems to have been generally assumed that since the tropics are hot there is more sunlight there, but the problem is

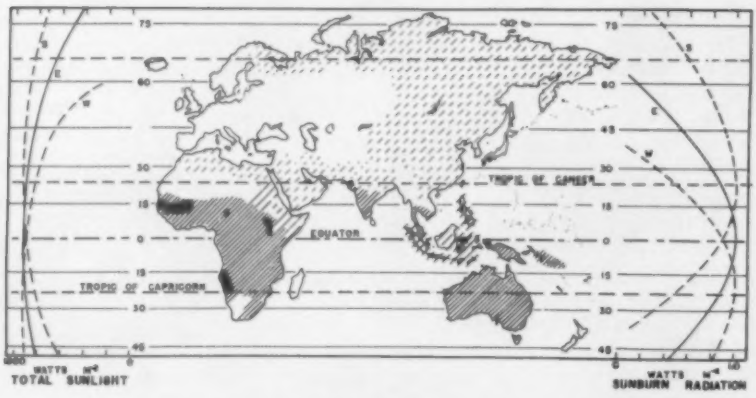

FIG. 2. Map of the distribution of brown skin color (After Fleure, 1945)

On the left, distribution of total sunlight with latitude. E, at equinox; S, at summer solstice; W, at winter solstice. On the right, distribution of sunburning radiation (wavelength shorter than 0.32μ) with latitude. E, at equinox; S, at summer solstice; W, at winter solstice. The curves on the right and left are based on values from Moon (1941). Note that the curves for summer and winter solstices are distorted by the Mercator projection. These curves, based on light incident upon a surface normal to the sun's rays, neglect scattering from the sky, which may be very important in the case of the sunburning radiation, and also neglect geometrical relationships of the human body profile with respect to the radiant environment. They are thus to be considered only as rough guides.

not so simple as this. Reference to Fig. 1, A will show that total sunlight decreases less rapidly as the sun moves away from the zenith than does the sunburning portion. Obviously the solar heat load, being related to total sunlight, must vary differently with latitude than does the sunburning radiation.

Fig. 2 gives a rough, general idea of the distribution of sunlight with latitude. Represented on the left of the map are distributions, with latitude, of the total energy of sunlight at noon under three conditions: (W) with the sun at its northernmost excursion (summer solstice), when it is over the tropic of cancer (23° 44′ N); (S) with the sun at its southernmost excursion (winter solstice) when the sun is over the tropic of capricorn (23° 44′ S); and (E) with the sun over the equator (equinox). On the right of the figure are represented the distributions of the sunburning fraction of sunlight at the same respective positions of the sun. It is noted that the sunburning radiation is much more "concentrated" in terms of latitude than total sunlight; the amounts of energy are, of course, very different in the two cases.

It is clear from the diagram on the left, that at some times of the year a part of one temperate zone may receive more total sunlight than does a part of the tropic zone; indeed, the difference in this regard between the temperate and tropic zones is not so great as is commonly conceived. Even the Arctic and Antarctic zones receive a considerable amount of solar energy during parts of the year. So it is seen that the effect of the solar heat load in exacerbating collapse due to water loss should not be restricted to the tropics—at the time of the summer solstice, for example, other things being equal, it should be just as likely to occur at 47° N (about the latitude of Zürich or Quebec) as at the equator. The fact that the sun crosses the equator twice during the year, but reaches the tropic of cancer only once, should not be of importance in this regard, since "heat stroke" is an acute effect, that is, one that involves, at most, a few days' exposure to the sun. Much more important would be other aspects of the environment such as desert conditions, which are to be found in the temperate zones as well as in the tropics. As has been said, high melanin content of the skin should be a slight disadvantage under those conditions. But it could be of limited advantage in a cooler climate where heat gain might be desirable.

In a recent paper, Cowles (1959) stresses the advantage of black or brown pigment in reducing visibility, and suggests that this might have had survival value for Man, in the role either of predator or pursued, at some time in his evolution.

Whatever the importance of this factor in natural selection, it would seem to be one which should be related more to local environmental situations than to a global distribution such as has been supposed as regards adaptation of Negro skin to life in the tropics. From the curves in Fig. 2 it may be seen that the visible portion of sunlight, which is that part pertaining to Cowles' thesis, varies relatively little in its maximum intensity, from Tropic to Temperate zones, or even the Arctic, where summer sunlight is bright. On the whole it would seem that Cowles' argument should apply more to noctural vs. diurnal life, than to geographical distribution.

It is to be noted from the curves on the right of Fig. 2 that the sunburning radiation varies more with latitude than does the total solar heat load. But nevertheless part of the temperate zones receive relatively high amounts at certain times of the year. At summer or winter solstice the chances of being sunburned should be about as great at 47° N or S, respectively, as at the equator, and with sufficient exposure sunburn may occur above the Artic Circle. Of course, sunburn is an acute effect, and the situation should be somewhat different as regards the "chronic" effects, carcinogenesis and antirachitic action—because these depend on the total exposure to the sunburning radiation throughout the year, and the sun is near zenith in the tropics for a greater part of the year than it is in the temperate zones. But, nevertheless, parts of the temperate zones receive a relatively high annual incidence of the sunburning radiation.

In the United States the occurrence of skin cancer within the white population shows a north-south distribution which fits reasonably well with estimates of the distribution of the sunburning radiation, within the latitude range (32° to 41° N) (see Blum, 1959). But at most the incidence of these cancers is small, and, as has been said, probably could have little effect on racial survival. The Negro population has a very low incidence of skin cancer, but I think no one would venture to say that this in itself has played a significant role in the relative distribution of the Negro and White populations in our country. Fairly recent historical events have been obviously much more important in determining this distribution than has natural selection. Would it not seem reasonable to think that, correspondingly, at a much earlier time events not directly related to the environment played a dominant role in determining the distribution of the Negro and White races in the eastern hemisphere rather than biological adaptation to the climate of the particular areas they now inhabit?

As regards historical factors in the distribution of races, the Bushmen of Southern Africa offer a most interesting example. These people, at present restricted to the region of the Kalahari Desert, occupied a much greater area, including quite different terrain and climatic conditions, at the time of the entrance of Europeans into this region. Subsequently the Bushmen have been pushed gradually out of other areas and have taken refuge in the Kalahari. Here the remnants of the race live under very difficult conditions in an arid land with relatively sparse vegetation. They appear to have adapted themselves to a precarious existence under these conditions by learning to take advantage of what corners of the environment they may grasp, rather, so far as we know, than by any particular physiological adaptation. If we read the record right, they have lost at least one remarkable facet of their previous culture in doing this, the making of the rock paintings that have been attributed to them—not a surprising loss considering the complete preoccupation with survival which seems to be their present lot. (See Willcox, 1956; Van der Post, 1958; Thomas, 1959). A cultural change resulting from recent historical events imposing a new habitation area would thus seem to account for the Bushmen's present adjustment to a rigorous environment, rather than selection on a biological basis. May a similar situation not affect other distributions and cultures where the history is unknown, being farther in the past?

My information regarding the Bushmen has been greatly increased since this paper was first written, by conversations with Dr. B. Kaminer, who has studied these people at first hand in the Kalahari, and has taken color photographs of them. Dr. Kaminer pointed out to me by means of his photographs, the great amount of dirt and desert soil which these people, who have no opportunity to bathe, carry about on their skins. Even without physical measurements one can be quite sure, I think, that the amount of sunburning radiation that reaches the skin is greatly reduced, and consequently the importance of melanin as protection in this regard. Seeing these photographs one might conclude that the solar heat load should be determined more in terms of the reflectance of the soil from the local terrain, than

in terms of that of the uncoated skin. The fact that the Bushmen tend to be brown rather than black would not seem of great importance. This observation seems still further to confuse the question of the adaptive value of melanin in "feral" Man or his ancestors, making its importance even more questionable.

Finally, we may ask whether the distribution of the dark-skinned races really fits very well with the distribution of sunlight. The comparison in Fig. 2 does not seem to give very strong support. The map shown there is drawn after Fleure (1945), who has attempted to explain the distribution of skin color in part in terms of local climatic conditions, but his argument does not take into account the nature of the physiological factors concerned. Actually, the comparisons made in the present paper, regarding melanin pigment and physiological effects of sunlight, are based on what is known about White and Negro skins, since applicable data are only available regarding these; to what extent the brown-skinned peoples may fall into an intermediate position with regard to the physical and physiological factors that have been discussed we do not know.

A HISTORICAL NOTE

In 1871, Darwin wrote in his *Descent of Man* (p. 200 in the Appleton edition of 1913), as follows: "Dr. Sharpe remarks, that a tropical sun, which burns and blisters a white skin, does not injure a black one at all. . . . I have been assured by a medical man that some years ago during each summer, but not during the winter, his hands became marked with light brown patches, like, although larger than, freckles, and that these patches were never affected by sun-burning, while the white parts of his skin have on several occasions been much inflamed and blistered. . . . Whether the saving of the skin from being thus burned is of sufficient importance to account for a dark tint having been gradually acquired through natural selection, I am unable to judge."

The complete passage is of interest, but to conserve space I have quoted only those parts that seem particularly cogent here. It is to be noted that Darwin was properly cautious about his attribution of dark skin to natural selection. The evidence of association of relative immunity to sunburn with pigmentation we need not question, although, as is not infrequently the case, Darwin is using information at secondhand. But one may question the interpretations, while remembering how little was known at the time about the action of sunlight.

The case of apparent immunity of pigmented patches on white skin to sunburn, is intriguing in the light of present knowledge. Was this, perhaps, a case of *vitiligo*?

Although Ritter had shown the existence of ultraviolet light in the sun's spectrum in 1801, only about the middle of the century was it realized that sunburn is caused by such radiation. This came about with an observation by the physicists Foucault and Despretz who were accidentally sunburned by the radiation from an electric arc with which they were experimenting; the matter was reported in a brief note by Charcot (1858). It had been thought before that time that sunburn resulted from heating the skin, and Darwin may still have been under this impression when he wrote. A more definitive demonstration that sunburn is due to ultraviolet light was reported by Widmark in 1889. Some of the more recent developments have already been noted.

Another early suggestion that skin color favors the residence of Negroes in the tropics is of interest. In 1887, Wedding presented, in a brief note before the Berliner Gesellschaft für Anthropologie und Urgeschichte, what appears to have been the first account of buckwheat poisoning in sheep, giving proof that sunlight is a precipitating factor. He showed that animals fed on the buckwheat plant developed lesions of the skin when subsequently exposed to sunlight, but that areas of the skin which he covered with tar did not display the lesions. We know today that this condition is brought about by a substance present in the buckwheat plant. When the plant is ingested, this substance gets into the blood stream and is carried to the skin, which it photosensitizes to wavelengths in the visible spectrum. Man seems never to eat enough buckwheat to become photosensitized in this way, and the basic photochemical mechanism is not related to sunburn (see Blum 1941, 1950). Clearly, the phenomenon observed by Wedding has nothing to do with the normal responses of human skin to sunlight, but he was unaware of this—very little being known about such responses at that time, particularly with regard to spectral relationships. He suggested that the pigment in the Negro skin offered protection in the same way as did the tar he painted on his sheep, and that this was of advantage to the

Negro in the tropics. He even went so far as to suggest that a ship be sent to the tropics with half the crew painted with walnut stain, for the purpose of studying the effects of tropical sunlight. His paper makes no mention of Darwin or natural selection.

I have often wondered how Wedding's paper came to be presented to an anthropological audience rather than to veterinary scientists. It described a discovery of first importance to the latter, but introduced only a misconception with regard to the study of Man.

GENERAL REMARKS

In examining the interrelations between certain physiological responses of Man and a particular facet of his environment—sunlight—we have found the situation more complex than it is generally thought to be. This particular environmental factor has more than one separate physiological effect on Man, and other environmental factors may play important roles in the overall result. The specific thesis examined—that high melanin content of the skin adapts to life in the tropics because it offers protection against sunlight—seems to have little to recommend it. The melanin might provide a degree of protection in some cases but could be detrimental in others. On the whole, one may doubt that the possession of melanin pigment makes very much difference one way or the other, and that it could serve as an important "handle" for natural selection seems most unlikely. And when we examine the distribution of sunlight, we find that the geographical distribution of skin color really does not correspond at all well with the suppositions of the thesis.

The particular point may not seem worth the laboring I have given it, and the removal of the notion from our thinking might not, in itself, affect very greatly our concepts of races and their origins. But certain doubts may be raised. We may be led to ask whether there are not other instances where survival value has been attributed to anatomical or physiological factors which it would be difficult to substantiate, or where pertinent aspects of the environment are not well understood. The case of adaptation of warm-blooded animals to cold climates, where Scholander (1955) has pointed out that the physiological and environmental factors concerned do not coincide with commonly accepted ecological and evolutionary ideas, seems another cogent example. Surely we may question the evidence in such cases without challenging the basic role of natural selection—I believe no one who has read my writings on evolution would accuse me of making such a challenge (e.g., Blum, 1955b, c).

What we really deal with here is the relationship between phenotype and environment, as I have said earlier, selection depending upon the closeness of fit between the two. Complete fitting is not to be expected in the complicated situations that prevail in nature. Rather, one may think of the over-all agreement as the algebraic sum of many positive and negative values, representing advantageous and disadvantageous facets of the phenotype with respect to corresponding facets of the environment. If the sum of these values is positive, the phenotype, and consequently the genotype, should have selective advantage. The total balance sheet could contain many plus and minus values, large and small; the relationship of melanin to sunlight seems one of minor weight in the survival of human races. It might be extremely difficult to analyze the situation accurately, and one could go badly astray by underestimating the complexity of the factors concerned.

If a somewhat facetious illustration is permitted, I may point out that if we consider only the role of melanin pigment with regard to sunlight, Negro skin would seem to be highly adaptive to life on one of the snow-capped mountains near the equator. Here the sunburning radiation should be high, being enhanced by reflection from the snow field, and the protection afforded against it might be assigned a high positive value. The low reflectance of total sunlight from the skin could be also advantageous in this situation in maintaining body temperature by increasing the solar heat load. The importance of antirachitic action, whatever its sign, would seem to be minor compared to the general problem of maintaining adequate nutrition. The negative factor of concealment might also be slight because there would be so little wild game anyway. I am afraid that in spite of the positive values assigned to melanin, the over-all fitting between phenotype and environment would have a rather high negative value in this case.

It seems so easy to read adaptive value into almost any aspect of an organism, particularly if our understanding of both physiology and environment is incomplete, as must so often be the case. If we yield to the temptation to find adaptive

value in every racial or specific characteristic, do we not run the risk of reaching a position not unlike that of Dr. Pangloss—in spite of Candide's observations, and our own?

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THE ADAPTATION OF FLOWERING PLANTS TO NOCTURNAL AND CREPUSCULAR POLLINATORS

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This paper was presented to a symposium on "Adaptation in relation to the speciational process" jointly sponsored by the Ecological Society of America and the Division of Comparative Physiology of the American Society of Zoologists at the AIBS Meetings, Stillwater, Oklahoma, on August 29, 1960.

ABSTRACT

The evolutionary significance of the adaptation of flowers to distinct classes of pollinators is considered and an attempt is made to assess the frequency with which the mutual adaptations of flowers and particular night-flying flower-visitors have become so close as to provide reproductive isolation for the plants from those adapted to other visitors. Probably the obligate relationship shown by Yuccas represents a nearly unique situation which may have reduced rather than promoted rates of speciation in both the Yuccas and the Yucca moths to which they are adapted.

A description is given to the general characteristics of flowers which are adapted to pollination by evening, night, and early morning visitors (including birds). The flowers of many tropical and subtropical trees are pollinated by bats, and two flower-types are discussed in this context. The restriction of bat pollination to the tropics and adjacent subtropics appears to result from a lack of suitable plant material in temperate regions.

Adaptations to combinations of flower visitors are considered in relation to risks of failure in seed-production. Such "bridging" may also have considerable significance in the evolution of adaptation to new pollinators. Finally, it is concluded that, as yet, there is no evidence that adaptation to any crepuscular or nocturnal pollinators is sufficiently complete to initiate speciation, although the partial reproductive isolation which it provides may complement the effects of other more primary factors in the maintenance of specific distinctions.

INTRODUCTION

THROUGH the pioneering efforts of Darwin (1876, 1877, etc.), the naturalists of the last quarter of the nineteenth century were given good reason to believe that cross-pollination was beneficial to most flowering plants and that there were, as a result, distinct advantages to the plants in the attraction of potential pollinators.

As a result of this stimulus, the naturalists searched for, and satisfied themselves that they had found, adaptive advantages in every morphological and behavioral feature of flowers and inflorescences. Along with this went hours of diligent observation, so that long lists of flower-visitors were recorded in which, however, casual and ineffective visitors were not always distinguished from those which regularly acted as pollinators. Thus, the Old Testament of floral biology was written. Knuth (1906–1909) brought it together in summary form.

The first half of the twentieth century witnessed a reaction against the enthusiastic Darwinism of the Victorian naturalists, and there was more or less of a hiatus before the New Testament began to be composed in the last couple of decades. There is, however, a radical difference between the accounts now being written and those dating from the nineteenth century. Great emphasis is now put upon the relationship between the flowers of individual taxa, on the one hand, and particular pollinatory animals on the other. Thus, individual plant species are classified as having "moth-flowers," "bat-flowers," or "bumble-bee flowers"—almost as if they had no other visitors. Adaptation of two plant taxa to different pollinators is held to provide a reproductive isolation which may be significant in the origin and maintenance of specific distinctions.

Nevertheless, there is need for caution. Before we can estimate the importance in evolution and speciation of adaptation to various pollinators, we need to know just which features of the flowers are adaptive, and how closely they restrict the flowers to a single pollinator.

RESTRICTION OF FLOWERS TO A SINGLE POLLINATOR

Restriction to a single pollinator certainly can be found. The Yuccas of the southwestern deserts provide us with an example in their obligate relationship with the Yucca moths—moths which used to be well known as species of *Pronuba* and are now known by the less familiar name, *Tegiticula*. Here, as is well known, the female lays her eggs in the ovary of the *Yucca* flower by penetrating the ovary wall with her ovipositor (Riley, 1892). She then pollinates the flower by pressing specially prepared balls of pollen into a cavity formed by the stigmatic portions of the style. Although the growing larvae eat a proportion of the developing seeds, nevertheless the remainder escapes being devoured and reaches maturity. The larvae of *Tegiticula* are completely dependent upon the *Yucca* plants which, in turn, have no other pollinators.

So close has this obligate relationship become that it even seems that the emergence of the moth from the chrysalis and the bursting of the *Yucca* flowers into bloom may be controlled by the same environmental factors. In this case it is worth examining the evolutionary consequences of such a close relationship.

Thanks to the earlier workers (Riley, 1892; Trelease, 1893), the diligent collecting work performed by McKelvey, and identification studies by Busck, we now know that there are no more than five species of *Tegiticula* engaged in the pollination of *Yucca* plants (McKelvey, 1938–1947)—and probably this number should even be reduced to three. One of these species—*Tegiticula yuccasella*—seems to visit nearly all of the *Yucca* species.

With merely three or five species of moth visiting about thirty species of *Yucca* there can only be one consequence—extensive inter-specific hybridization; and, of course, this is just the problem which has plagued taxonomists who have attempted to deal with this desert genus (McKelvey, 1938–1947; Webber, 1953, 1960; etc.).

There may be a further consequence for *Yucca* and *Tegiticula* which, despite this interbreeding, has kept the roster of *Yucca* species down to only thirty and those of the moth down to a number countable on the fingers of one hand. The relationship between plant and moth is now so close that any physiological variation by either party could throw the arrangement out of gear and result in the failure of the deviate to reproduce. Consequently, even though we must generally agree with Grant (1949) that adaptation to discriminating insect-visitors may accelerate speciation over the rates which prevail in groups where pollen is distributed in a more nearly random manner, too close a relationship may have a restrictive effect on speciation.

Yucca has some thirty recorded species (McKelvey, 1938–1947). *Agave*, a genus with a geographical distribution comparable in extent and a similar ecological amplitude, has about 275 species (Munz, 1959). It may not be coincidental that *Agave*, as a genus, is visited by a range of pollinators from bees to birds and bats (van der Pijl, 1936; Porsch, 1939; Cockrum, unpub.; Baker, unpub.).

However, it may be that *Yucca* is the glaring exception. Just how common *are* 1:1 relationships of this sort? In fact, it seems that they are extremely rare—and cases which have been described as approximating to this often fail to live up to their reputations when they are examined closely. We might take the case of the bindweed, *Calystegia* (*Convolvulus*) *sepium*, as an example. In 1873, White wrote about this species in the *Journal of Botany* and his article was reprinted in the *American Naturalist*. He pointed to a rough coincidence between the geographical distributions of the bindweed and of the Convolvulus Hawkmoth (*Sphinx convolvuli*), and suggested that this is because the moth is the outstanding pollinator of the bindweed. Where the hawkmoth cannot at least penetrate by annual migration, the bindweed cannot reproduce.

This apparently significant relationship quickly found its way into the textbooks (cf. Knuth, 1906–1909), and it has been repeated right up until recently. However, there has been a distressing paucity of firsthand records of the evening- or night-flying Convolvulus Hawkmoth actually being seen visiting *Calystegia sepium*. In fact, the flowers do not seem very well adapted to receive it as a regular pollinator—for they are scentless and largely diurnal.

Observations and experiments (Baker, 1957) have shown that these bindweed flowers are host to a considerable number of daytime visitors—honey bees, bumble bees, syrphids and other Diptera (Fig. 7)—and that these are quite capable of acting as pollinators. The failure of the species to produce seed at the limits of its distribution range is apparently due to the very vigor of its

vegetative reproduction, which leads to the establishment of single clones over a wide area. Being self-incompatible, these clones then have no capability of forming seed.

Thus it seems that we can be in danger of drawing false conclusions if we take too hasty or too narrow a view of the pollination systems of flowering plants. For this reason, we should now consider the general characteristics of plants which are pollinated in the evening, or at night, or in the early morning.

ORNITHOPHILOUS FLOWERS

There is a surprisingly large range of pollinators which needs to be examined here and, in fact, we may begin with birds. After all, there is a record (for whose accuracy it is not possible to vouch) of the Costa hummingbird hovering over Southern California flowers in bright moonlight (Fisher, in Bailey, 1902). Whether this is valid or not, it is unquestionably true that hummingbird activity is greatest in the evening and again in the morning. This can be related to the tremendous metabolic activity of these vigorous little birds and to their inability to build up food reserves. A good, carbohydrate-rich supper and a good breakfast are essential to their well-being.

Of course, in addition to the hummingbirds of the New World there are other nectar-drinking birds in the Old World, birds which do not have the hovering abilities of the hummers. The Nectariniidae, Zosteropidae, Meliphagidae, and the extinct Hawaiian Drepanididae fit into this category.

Ornithophilous flowers share a number of characteristics. In general they have tubular corollas, with plenty of thin nectar produced at the base. The stamens and styles may be exserted and, in any case, plenty of pollen is produced and is of such a nature that it coheres readily to the beak or head feathers of any visiting birds. The flowers are held in such a position that they can be probed by acrobatic or hovering birds, as the case may be. They lack discernible scent.

Of great importance to our present discussion are the colors of ornithophilous flowers—which are commonly red, yellow, or white, or show various combinations of these colors. Birds are particularly sensitive to reds and yellows, but there is probably more to it than merely this.

As the sun gets near the horizon, and after it has sunk beneath the horizon, the light has special richness in those wavelengths which are at the red end of the spectrum. In these circumstances, red and yellow flowers may stand out brilliantly against the correspondingly darker green of bracts and leaves, and may be particularly attractive to the birds which are especially active in flower visitation at just this time.

CHIROPTEROPHILOUS FLOWERS

The second group of plants to be considered is made up of those whose flowers are visited by bats—that is to say, plants which are chiropterophilous. Although these plants are often taxonomically very closely related to those which are ornithophilous, so that an evolutionary development of one from the other is very likely, they show characteristic differences. Bat-pollinated flowers are generally dull in color or are white, and, flying after dusk, vegetarian bats appear not to rely upon sight as much as upon their sense of smell in the location of suitable flowers. A musty or fruity smell characterizes bat flowers and is produced along with copious quantities of nectar when the flowers open at or after dusk.

Basically, chiropterophilous flowers may be divided into two groups, related to some extent to the sizes of the bats which visit them. The first group may be illustrated by the Baobab, *Adansonia digitata*, a famous African tree of the Bombacaceae (Fig. 1).

In this species, the large, creamy white, odoriferous flowers are pendent on long, stout pedicels, so that they hang clear of the branches. Visiting bats hold on by clasping the ball of stamens to their breasts while lapping nectar from the base of the stamen-column. In the process, their breasts become covered with pollen which may be transferred subsequently to the stigma which projects to one side of the flower on the widely diverging style (Harris and Baker, 1959). The bat which visits baobab flowers is a large one, *Eidolon helvum*. Its wing span may be as much as a meter, but it is a strict vegetarian.

Variations on this pattern, in which a ball or brush of stamens is exposed to the breasts of clasping bats, are to be seen in other members of the Bombacaceae as well as in other families like the Myrtaceae. In the case of another common African tree of the Bombacaceae—the Kapok tree, *Ceiba pentandra*—the "brush" of stamens is provided by a number of flowers clustered together (Baker and Harris, 1959). The flowers open after dark and then glisten whitely with large quantities of nectar.

These flowers are also visited by *Eidolon helvum*, which crawls over and among them lapping nectar, chewing stamens and, incidentally, effecting pollination. Two other species of bat behave similarly on these flowers in West Africa.

A more extreme indication that the "stamen-ball" mechanism can be reproduced by inflorescences made up of small flowers is given by members of the Mimosaceae. *Parkia clappertoniana*, a savannah tree from tropical West Africa, can serve to demonstrate this (Baker and Harris, 1957).

It is the inflorescence, in this case, which hangs free from the foliage (Figs. 3–5). The inflorescence itself shows a division of labor, with staminate flowers on the cylindrical portion producing large quantities of nectar. This runs down and collects in a circular depression on top of the ball of potentially fertile flowers.

The inflorescences are visited after dusk, first by bats of the species *Epomophorus gambianus* and, later, by a smaller bat, *Nanonycteris veldkampii*. The bats clasp the ball of flowers, lapping nectar from the circular depression. Strong protandry prevails in *Parkia*, so that most of the flowers are in a staminate condition one night and in a pistillate condition the next night. Cross-pollination takes place as pollen is carried on the fur of the bats.

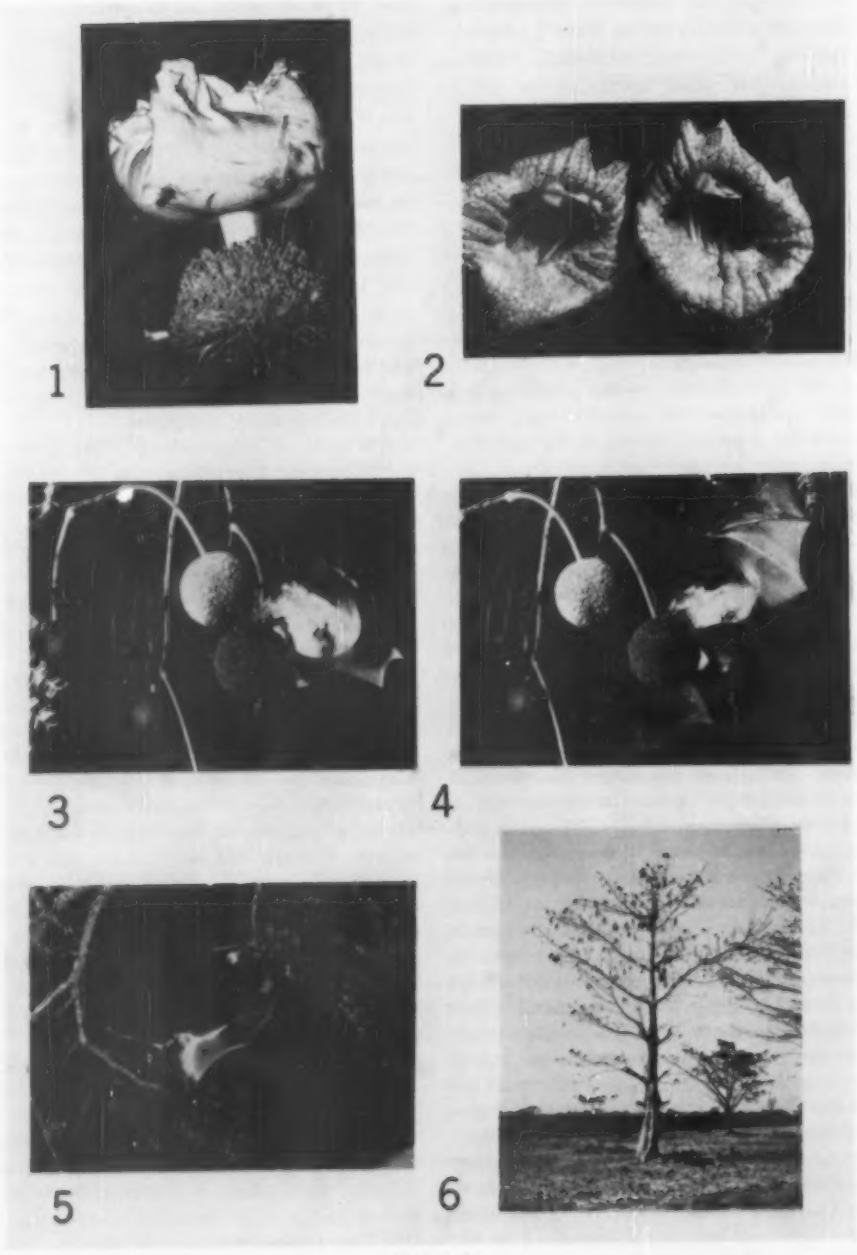
The second basic type of bat flower is demonstrated by those families which have gamopetalous corollas. These form bell-shaped flowers which need to be entered before the nectar can be reached or pollen-transfer can be effected (Fig. 2). Thus, only smaller bats can visit these flowers. The Bignoniaceae is particularly well represented among bat flowers of this type, and the Sausage Tree (*Kigelia africana*) may serve as an example, again drawn from West Africa. In this tree, the inflorescence consists of a pendent raceme with each flower opening in a horizontal plane. Closed during the day, the flowers open just after sunset. When the bats, which are *Micropteropus pusillus*, visit them, they land on the lip of the corolla and push their heads into the corolla-tube, depressing the flower and tipping the nectar towards them. In the process, they receive pollen on their backs—pollen which may be transmitted to the similarly placed stigma of another flower (Harris and Baker, 1958).

Pollination of flowers by bats is characteristic only of tropical regions, with some extension into the subtropics; but there is an interesting difference between the bats which are responsible for this in the Old World and in the New World tropics. Of the two suborders of bats, it is the Megachiroptera which is exclusively concerned with flower-visitation in the Old World (Allen, 1940). As nearly as one can reconstruct the picture, however, the migration of bats to the New World was through temperate, boreal regions and was accomplished by insectivorous members of the second suborder, the Michrochiroptera (Baker and Harris, 1957). Once in the New World tropics, the Microchiroptera have radiated out and filled the ecological niche which is occupied by the Megachiroptera in the Old World. As yet, they seem to be less completely specialized than their Old World megachiropteran cousins and still take insects in their diet, in addition to nectar and pollen.

The question is immediately raised as to why bat-pollination appears only to be possible in tropical regions. The bats appear to have been adaptive enough to take advantage of suitable flowers wherever they have found them. Consequently, it is most likely that the answer is to be found in the nature of the plants available in tropical and temperate regions rather than in some function of the bats' activities.

Bat-pollinated plants need to be sturdy and generally have a tree-like form. This is necessitated by the weight and flying-habits of the bats. However, trees with large, nectar-producing flowers or inflorescences are not common in temperate regions (where most trees are wind-pollinated), but they are quite common in the tropics of both hemispheres. Similarly, the need for free flying space around the flowers or inflorescences necessitates the production of these flowers on long, stout stalks which, in turn, must hang clear either from the trunk or main branches or from the tips of drooping branches. Cauliflory and flagelliflory of this sort are also characteristics of tropical trees (van der Pijl, 1956).

Furthermore, the development of the branch-systems of the trees in whorls along the main trunk allows more room for the large, flying bats—and this "pagoda" habit (Fig. 6) is yet another characteristic of tropical, rather than temperate trees (Corner, 1940). Lastly, it is essential for the nutrition of such a large animal as a vegetarian bat that food be available over an extended period. Here, again, tropical trees have the advantage over the more specialized temperate trees, for not only may a succession of individual trees of the same species often be found in flower throughout



FIGS. 1–6

much of the year, but even a single tree may have a prolonged flowering period (Baker, 1959). Flowering in trees of the temperate regions is generally very restricted in duration.

SPHINGOPHILOUS FLOWERS

Pollination by moths is, perhaps, a more generally familiar phenomenon and needs relatively little description now. Nevertheless, perhaps it should be emphasized that not all moth-pollination takes place after sunset. The honeysuckle, *Lonicera periclymenum*, is visited in Europe during the day by the hawkmoth *Macroglossa stellatarum* and at dusk by other hawkmoths of the genus *Sphinx* (Knuth, 1906–1909).

Generally, however, such sphingophilous flowers are characteristically night-opening, and with long, narrow corolla-tubes or spurs at the base of which nectar is secreted. The flowers are generally believed to be white or creamy in color with a sweet penetrating odor which is emitted only in the evening or at night. *Nicotiana longiflora* exemplifies the classical moth flower.

Despite this, it should be pointed out that a white flower color is not, by any means, essential for the attraction of moths. *Oenothera hookeri* has pale yellow flowers—and in *Amaryllis belladonna* they are pink. In the reddish light of evening both of these colors stand out brightly against a green background and the amount of pigmentation is not so great that the flowers will be invisible even when the light intensity has diminished to an extremely low level. The *Amaryllis* has flowers which remain open day and night, while in the *Oenothera* they open around dusk.

Of the moths which visit flowers, those most frequently watched are the hawkmoths, and it is commonly assumed at least by botanists, that these always hover, much in the manner of humming birds, in front of the flowers which they probe with their long proboscises. While this may be true with pendent flowers, the moths often rest upon a corolla-lip if this is provided by flowers which stand horizontally or erect. Where, as is often the case, the style and stamens project from the flower, the moth may rest its underside upon them, carrying out pollination in the process.

Where the flower-tube is large enough, even a hawkmoth will push its way right inside, as pictures of *Phlegonthius sexta* visiting *Datura meteloides* in California show (Figs. 8–10).

It has to be recognized, however, that in many climatic situations, complete dependence upon moths for pollination is a risky business. In areas where summer nights may be cool, hawkmoths fly much less on such occasions than on warm nights.

Even when the moths do fly, their numbers may be relatively small and many flowers may never be visited. Perhaps, therefore, it is not surprising that various kinds of insurance mechanism appear to be exhibited by those species in which moth-pollination is found.

Thus, there may be good vegetative reproduction (as in the *Amaryllis* already mentioned) or, at least, longevity in the plant with the production of a large number of flowers during its life-span. When pollination is successful a large number of seeds may be produced by a single flower—the genus *Nicotiana* provides a good example of this. Finally, although a number of moth-pollinated plants are self-incompatible, there are at least some which may take refuge in self-fertilization in the absence of successful moth-visits. Thus, *Mirabilis longiflora* sets seed by self-pollination in our Berkeley greenhouses (Baker, unpub.). By contrast to moths, bats appear to be quite consistent in their flower visitations and, in West Africa, they

FIG. 1. FLOWER OF *Adansonia digitata* (BOMBACACEAE)
Photographed in the morning at Weija, Ghana, showing claw marks left by *Eidolon helvum* during visits made the previous night. Photograph by Dr. B. J. Harris.

FIG. 2. FLOWERS OF *Parmentiera cereifera* (BIGNONIACEAE)
Photographed at night showing the open-throated structure suitable for bat-visitation. The sensitive stigma-lips are visible alongside the anthers. Photograph by Dr. B. J. Harris.

FIGS. 3 AND 4. *Epomophorus gambianus* VISITING INFLORESCENCES OF *Parkia clappertoniana* IN GHANA
The balls of flowers are gripped by the feet of bats while nectar is lapped from the depressed ring in which it collects. Photographs originally published by Baker and Harris (1957).

FIG. 5. *Nanonycteris veldkampii* FLYING BETWEEN INFLORESCENCES OF *Parkia clappertoniana* IN GHANA
The production of the spherical inflorescences on stout peduncles hanging from major branches allows flying room for bats.

FIG. 6. YOUNG FRUITING TREE OF *Ceiba pentandra* GROWING NEAR TAMALE, GHANA
The "pagoda" habit of growth is clearly shown and is of importance in allowing ready access of bats to the flowers which are borne in clusters on the temporarily leafless branches.

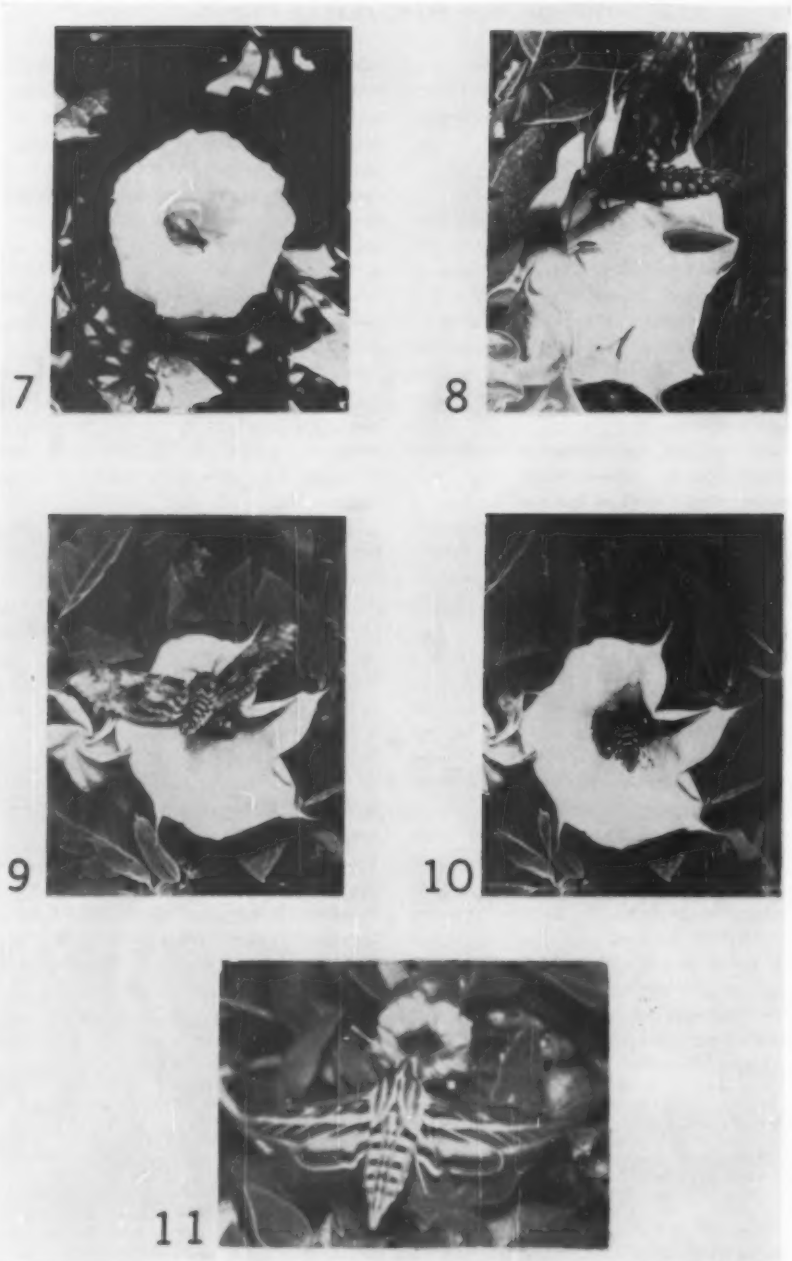


FIG. 7. FLOWER OF *Calystegia* (CONVOLVULUS) *sepium* GROWING NEAR BRIGHTON, ENGLAND
Photographed at 10:30 A.M. with a syrphid (*Rhingia* sp.) visitor.

FIGS. 8–10. VISITS OF *Phlegonthius sexta* TO FLOWERS OF *Datura meteloides* AT DAVIS, CALIFORNIA
Photographs made during twilight in September. The moths will push right into the flower to get nectar and carry pollen on the undersides of their bodies.

FIG. 11. CELERIO LINEATA VISITING *Mirabilis froebelii* AT BERKELEY, CALIFORNIA
Photograph taken at 8:30 P.M. in June

have been seen flying on their rounds even during a rainstorm.

But this question of the risk of failure in seed-production which pertains particularly to moth-pollination once again raises the question of whether adaptation to a single class of pollinators is even of frequent occurrence, let alone any kind or rule. Therefore, attention should be drawn to some of the *combined* systems which can be found.

COMBINED POLLINATION SYSTEMS

The first system combines the activities of birds and butterflies, and may be included here simply because it makes a point about flower color. *Lilium humboldtii*, for example, is visited in the daytime by swallow-tail butterflies and also by hummingbirds. Here, the orange-red color may have its full significance, for the flowers are brilliant by day and in the evening; they are virtually invisible, at least to human eyes, after dusk.

Even if scentless orange-red flowers stay open after dark, they are likely to remain unvisited—as can be seen in the West African Tropics from watching orange-red *Bombax costatum* flowers, which are very attractive to birds by day but remain unvisited at night (Baker, unpub.).

However, this does not apply to flowers in which the red has a blue component instead of a yellow one. Pink or rose-colored flowers, or even those with a light purple coloration, may be just as attractive to birds before twilight and yet be visited by hawkmoths at and after nightfall.

Nowhere has this been more obvious than in the case of *Mirabilis froebelii*, from southwestern North America. The flowers of this plant open at about 6:00 P.M. After this, when the nectar flows, the flowers become extremely attractive to hummingbirds. In cultivation in Berkeley during July, this activity continues until dusk at about 8:30 P.M. Quite sharply, then, the visits of the hummingbirds cease, and hawkmoth visits by *Celerio lineata* begin (Fig. 11).

Interestingly enough, the flowers of *Mirabilis froebelii* have no odor which is detectable by human nostrils, but the stems and foliage, covered as they are by glandular hairs, have a particularly strong odor at dusk. This is not the sweet odor usually believed to be attractive to moths, but is very comparable to the rank odor emitted by the foliage of tobacco plants. Nevertheless, it appears to be capable of attracting the moths from a distance and, as they fly in, they make sweeps over the plants before appearing to make a visual fix on a particular flower and flying to this one as the beginning of a series of flower-visits.

Occasional visits to flowers of *Mirabilis froebelii* are paid by other moths, but these are generally rather indeterminate in the manner in which the moths fly around the plants. After this, flower visits fall off during the night but, in the morning, while the flowers are still open, they are visited again by hummingbirds and then by nectar- and pollen-collecting bees, as well as occasional butterflies. Sharing the flowers by hummingbirds and hawkmoths may prove to be a widespread phenomenon.

In the Old World, it has also been possible to see the sharing of flower materials between moths and larger animals. This time it is bats which are involved—and again we may use *Kigelia africana* as a West African example. Immediately after dusk, the flowers of this tree, having barely opened, are visited by hawkmoths—in this case by *Nephele vau*. These moths do settle in order to penetrate to the nectar; and a photograph has been published in which two moths are visiting a single flower, one already in the flower and another lined up ready to enter, with its proboscis extended (Harris and Baker, 1958).

This particular case is of considerable interest because the flowers would hardly have been suspected of being attractive to moths on the basis of the classical image of a "moth flower." The flowers of *Kigelia* are deep purple in color while their smell, far from being sweet, is quite foul.

After a few minutes of activity the moths leave and the bats arrive. The moths, by reason of their smaller size, may not be as effective in pollination as the bats, but there is evidence from the Shai Hills area of Ghana that fruit-setting is possible in moth-visited *Kigelia africana* despite a local absence of bat-visits (Harris and Baker, 1958).

Among those plants which are pollinated by birds, bats, and moths, there are many which produce an abundance of pollen, which is prominently displayed. If this is available before dusk and again after dawn, it is ready to be taken advantage of by pollen-collecting bees, whose greatest activity tends to come at these times.

Thus, the drone of bees around flowering trees of *Adansonia* and *Ceiba*, before dusk or after dawn, is so conspicuous that the presence of the trees can sometimes be heard before they are seen. Toxopeus (1950), who studied *Ceiba* in Java, believed that

dawn-flying bees were its only pollinators, having missed the bats which fly after dusk. Similarly, the large nocturnal, white-flowered cacti which belong to several genera of the Cereae, from Central and South America, and are likely to be bat-pollinated in their native habitats, are freely visited by pollen-collecting bees before dusk and after dawn in cultivation at Berkeley.

However, where pollen-collecting bees visit plants with marked protandry these visits will have no significance for pollination; and this is the case with the bees which visit the bat-pollinated *Parkia clappertoniana* (Baker and Harris, 1957).

One difficulty which has always faced biologists who pin their faith in natural selection as an evolutionary force is how one pollination system can evolve gradually from another without an interregnum during which neither is effective. Perhaps these examples of adaptation to more than one pollinator, even when these are what are normally thought of as specialized types, may show how such an evolutionary change could take place gradually without the threat of extinction.

CONSISTENCY IN FLOWER-VISITATION

In assessing the adaptive value of various pollination systems, we must not only be concerned with how many potential pollinators visit a particular kind of flower, but also with how consistently the pollinator stays with the flower in question. Of the flower visitors dealt with here, the bees may be considered highly consistent, particularly when pollen-collecting (Grant, 1950), and the hawkmoths also have a reputation for consistency (Lovell, 1918). As for the bats, in West Africa we found *Micropteropus* only on *Kigelia*, *Eidolon* only on *Adansonia* and *Ceiba* (and never on both during the same evening), and the other bats were only slightly less consistent. Hummingbirds, on the other hand, show very low consistency unless there is a marked excess of one nectar-producing plant in the area. Thus, in Berkeley, a single individual of the Allen hummingbird (*Selasphorus alleni*) was observed to fly from the flowers of *Nicotiana setchellii* to those of *Phormium tenax* and from there to the blossoms of *Tropaeolum majus*.

DIFFERENTIAL ADAPTATION TO POLLINATORS AS AN ISOLATING MECHANISM

In conclusion, then, it appears to be justifiable to claim that the adaptation of flowers to various kinds of pollinators is a real phenomenon. Unlike Henslow (1891), we cannot now believe that this adaptation is the direct, inherited result of a chronic irritation of the flower by generations of the same animal visitors. Indeed, we have no evidence, at least as yet, that the origin of such adaptation is in any way more than completely fortuitous. Nevertheless, once originated, there is every likelihood of its refinement by natural selection. However, at least with pollinators of the types which we have been considering, it rarely appears that this refinement has gone so far that there is complete restriction to a single type of pollinator. We should keep this in mind when considering differential adaptation to pollinators as a factor in speciation.

There is one case in which it has been claimed that adaptation to two different pollinators, of the sort with which we have been dealing, is sufficiently exclusive to provide an isolating mechanism which plays a large part in keeping two closely related plant species more or less separate in nature. The data, in this case, we owe to Grant (1952), who made the observations in California on a pair of columbine species.

Aquilegia formosa var. *truncata*, with downward-pointing, short-spurred red and yellow flowers, is pollinated by hummingbirds, as might be expected. *A. pubescens*, on the other hand, has erect, long-spurred white or pale yellow flowers. This species is pollinated by twilight-flying hawkmoths.

Where these two species meet in the Sierra Nevada, they hybridize occasionally, but in Grant's experience, it needs an interloper to bring about the breakdown of isolation between them. Pollen-collecting bumblebees (which are the interlopers) may visit both species occasionally and make the cross. Introgression follows, largely because of back-crossing to the parental species effected by their usual pollinators. However, as a result of back-crossing to *Aquilegia pubescens*, pink-flowered plants can be found in the population and these may be visited by the hawkmoths as well as by the hummingbirds. Thus we may see how the isolation between the species can be breached even if it is not normally broken down, because their flowers are not exclusively adapted to a single pollinator.

Needless to say, there are more differences between these two columbine species than just those in the flowers, and a significant part of the isolation between them is provided by their adaptation to quite different physiographic and edaphic situa-

tions. Adaptation to two different pollinators of the sorts with which we have been dealing may never be sufficiently complete to *initiate* speciation, but this does not deny that the reproductive isolation which it helps to give may be valuable in backing up other, more nearly primary causes of speciation. Grant (1949) has already pointed out that the evolutionary role of this kind of isolation, even with discriminating day-flying insects, appears to be the reinforcement of allopatric speciation. There is a great need for careful observations in many more test cases before we can estimate the overall importance of this kind of isolation in the evolution of flowering plants. In such studies it will be insufficient to rely simply upon observations made during a restricted period of daylight. Visitors at any time when the flowers are open may be effective in cross-pollination and, of course, it is only observations made in a state of nature which are conclusive.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionally appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

THE PRINCIPLES OF SCIENTIFIC RESEARCH. *Second Edition.*

By Paul Freedman; introduction by C. H. Waddington. Pergamon Press, New York, Oxford, London, and Paris. $4.50. xviii + 228 pp.; ill. 1960.

The aura that has been placed around research by Madison Avenue has shaded the eyes of the public from the fact that for every successful experiment, there are ten failures. Not only experiments but also theories are uncertain. The experimeter is a professional doubting Thomas who cannot say that a theory is true but only that it has not as yet been disproven. As is evident from this book, Freedman is one who has done much research work in apprentice, journeyman, and master grades. He presents the theory and practice of scientific research in a book that neglects neither the joys nor the sorrows in life of the researcher.

The book is divided into 3 sections: (1) Development of the processes of research; its relationship with social change and available techniques; (2) The principles of the research process, types of problems, methods of attack and essential discipline; and (3) The support available for research.

Under these headings are discussed the relation between hypothesis and experiment, how to plan a research program, and the details of running a laboratory. Many examples of the types of problems encountered are drawn from Freedman's own laboratory experiences. Moreover, the author argues that the self-discipline of the researcher must be recognized, and he must be allowed to fix his own work schedule even to the extent of his daily time of arrival and departure! He also considers the much more delicate question of which people in the laboratory should be given credit for a particular piece of research. All researchers will recognize that Paul Freedman knows research and has written of it very well. I think it would not be too extravagant in praise to say of this book that it should be required reading for all graduate students who expect to devote any large part of their lives to research.

MYRON L. WOLBARSHT

BIOLOGY: HISTORY AND BIOGRAPHY

BIOGRAPHICAL MEMOIRS. *Volume XXXIV.*

The National Academy of Sciences of the United States of America; Columbia University Press, New York. $5.00. viii + 367 pp.; ill. 1960.

BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY. *Volume 5.*

The Royal Society, London. 30s. vi + 280 pp. + 19 pl. 1960.

Volume XXXIV of the *Biographical Memoirs* of our National Academy of Sciences, published in 1960, contains biographies of the following scientists, the years of whose deaths are indicated in parentheses: Werner Emmanuel Bachmann (1951), Reginald Aldworth Daly (1957), Bergen Davis (1958), George Augustus Hulett (1955), Charles Franklin Kettering (1958), Morris Selig Kharasch (1957); Otto Meyerhof (1951; this Memoir is reprinted from *Science*, volume 115, 1952), James Bumgardner Murphy (1950), John Ulric Nef (1915), Louis Valentine Pirsson (1919), Carl-Gustaf Arvid Rossby (1957), Florence Rena Sabin, the first woman ever elected to the Academy (1953), Lewis Buckley Stillwell (1941), John Reed Swanton (1958), and Willis Rodney Whitney (1958).

Volume 5 of the *Biographical Memoirs of Fellows of the Royal Society of London*, dated 1959 but published in 1960, contains biographies of the following scientists: William Ringrose Gelston Atkins, George Macdonald Bennett, Francis Joseph Cole, Cecil Henry Desch, Thomas Lydwell Eckersley, Alexander Gibb, Claude Dixon Gibb, John Jackson, Karl Spencer Lashley, Douglas Mawson, Samuel Roslington Milner, Robert Muir, Wolfgang Ernst Pauli, Ludwig Prandtl, Owen Williams Richardson, Meghnad Saha, John Lionel Simonsen, James Swinburne, and Frank Twyman. Prandtl died in 1953, Saha in 1956, and Simonsen in 1957, all the others either in 1958 or 1959.

Each of the memoirs, in both volumes, is accompanied by a portrait (for one Fellow of the Royal Society two, both a photograph and a drawing), and also a bibliography. The style of the articles of course varies from memoir to memoir, as well as from one volume to the other, and reflects whatever difference exists in habits of expression practised by British and American writers. While the National Academy volume has more pages than that of the Royal Society, the format of the two volumes differs both in the size of page and in the number of words per page, so that the total number of words is not very different in the two volumes. Our great men deserve evaluation of their memoirs in other than arithmetical terms, but the number of words in the two volumes has been calculated and mentioned here in order to underline the fact that excessive length is not the primary factor causing the great delays suffered by the publication of a number of the American memoirs. Those elected as members of our Academy, just as much as Fellows elected to the Royal Society, deserve prompt memorials; not only because fresh living memories furnish the stuff out of which living, and thus reliable, biographies are made, but also for the sake of decent respect.

JANE OPPENHEIMER

CONTRIBUTIONS FROM THE MUSEUM OF HISTORY AND TECHNOLOGY. *Papers 1 to 11. U. S. Natl. Museum, Bull. 218.*

Smithsonian Institution, Washington. $2.50. vi + 201 pp.; ill. 1959.

This volume of the *United States National Museum Bulletin* is the first collection of papers published by the staff of the Museum of History and Technology, recently established as a subdivision of the Smithsonian Institution's United States National Museum. The volume was planned by Robert P. Multhauf, head curator of the new subdivision's Department of Science and Technology, as a tribute to Greville Bathe and his contributions to the history of technology.

The 11 contributions are by Grace L. Rogers, Edward C. Kendall, Philip W. Bishop, Edwin A. Battison, Leslie J. Newville, Derek J. de Solla Price, Robert P. Multhauf, W. James King, Donald H. Berkebile, George B. Griffenhagen and James Harvey Young, and Jacob Kainen, all of whom were at the time of writing affiliated with the new Museum and planning for its future. Their subjects range through wool-carding machines, plows, steel production, watches, clocks, compasses and perpetual motion devices, phonographs, Conestoga wagons, mine pumps, patent medicines, and wood engravings. One of the 11 articles deals with the productions of mental rather than technological machinery, discussing William Gilbert's theoretical ideas and their relationships to those of his predecessors. This fortuitous proportion is probably consonant with popular belief as to the relationships of so-called pure science and technology, but the content of all the articles is quite otherwise. It is to be hoped that subsequent collections will maintain the high standards of the first.

The quarto volume, which is refreshingly inexpensive in ratio to its quality, is excellently designed and illustrated and is a fine example of technological skill on the part of the United States Government Printing Office.

JANE OPPENHEIMER

THE STORY OF CHEMISTRY.

By Georg Lockemann. Philosophical Library, New York. $4.75. viii + 277 pp. 1959.

This is a concise outline of the history of chemistry from its earliest beginnings in Egypt and Greece, four millennia ago, to developments in chemistry up to the middle of the twentieth century. The modern developments are scantily treated, however, and the major part of the book is devoted to the development of chemistry from the sixteenth to the middle of the nineteenth century. The work is encyclopedic in nature. The author, for example, has included items concerning the lives of the chemists

and considerable information concerning personal contacts between them. Much etymological information is also given. The book is recommended for those interested in a brief survey of the background of chemistry.

EMIL H. WHITE

HENRY E. SIGERIST ON THE HISTORY OF MEDICINE. *Edited by Marti-Ibáñez; foreword by John F. Fulton. MD Publications, New York.* $6.75. xviii + 313 pp.; ill. 1959.

HENRY E. SIGERIST ON THE SOCIOLOGY OF MEDICINE. *Edited by Milton I. Roemer; foreword by James M. Mackintosh. MD Publications, New York.* $6.75. xiv + 397 pp. 1960.

These two companion volumes present in the aggregate nearly 60 essays on the history and sociology of medicine. All of the 27 essays in the volume on *History*, and 28 of the 31 of those in the *Sociology*, have been published elsewhere. More than 30, that is, slightly over half of the essays in the two volumes, were delivered as addresses. Perhaps unfortunately, only 3 of the essays presented in the two books were written before Sigerist entered into the American period of his life and work. No excerpts from Sigerist's published books have been included. The essays first published elsewhere have been collected from many sources; only one appeared originally in the *Journal of the American Medical Association*. The stated objective of the volume on *Sociology* is to make available the principal Sigerist essays not already found elsewhere in book form, and it is clear, although not expressly stated, that this is also the aim of the historical collection.

The editor of the historical volume specifies that Sigerist himself chose a number of the selections now included in it. The two volumes are similar in format, and so far as the editing is concerned they differ only in minor detail. The volume on medical history includes a foreword by John Fulton, and an introductory essay on Sigerist's mind, by the editor. In this volume, two pages following the title page list the published sources of the essays, and footnotes on the first page of each article indicate when and where those that were addresses were delivered orally. The volume on sociology contains a Foreword on Sigerist as a medical sociologist by James M. Mackintosh, as well as a Preface by the editor. No separate list of references to the original places of publication is included in this volume, but the first page of each essay carries a footnote stating the place of publication or describing the manuscript source, specifying where, if at all, it was delivered as an address, and evaluating briefly how the subject of the article fits into Sigerist's life and thought. Each book has an index, and following this, a page including details of Sigerist's career.

Since to Henry Sigerist the history of medicine was the history of medicine in society, there is much that is common to the thought that imbues both volumes. The volume on *Sociology*, for instance, includes essays on the history of medical licensure, on the historical development of hospitals, on the history of dentistry; and the volume on *History* contains articles on the social history of medicine and on industrial and occupational diseases.

Sigerist's mind ranged far in time, and his activity in space. The sociological essays include studies on medical conditions and on the organization of medical care in England, Europe, Yugoslavia, the USSR, Canada, India, South Africa, Hawaii, South America, and New Zealand, as well as in the United States. The historical essays cover the medicine of ancient days (Babylonian and Egyptian as well as Greek), that of the Middle Ages and the Renaissance, that of the period of the Industrial Revolution, of our own day, and of the future. Nevertheless, their tenor of thought is hauntingly constant. Sigerist believed that poverty is the major cause of illness, and he viewed the state of medicine everywhere and in all periods as only one phase of development leading towards the ideal medicine of the future: a medicine with prevention as its primary aim, and with medical assistance free for all, governmentally organized and financed.

In one of the finest essays in the collections, the valedictory address delivered when he was about to depart from his position as Director of the Institute of the History of Medicine at the Johns Hopkins University, Sigerist quotes from a survey on the teaching of medicine in 1904 a wonderful reason for the study of history: "It is the best antidote we know against egotism, error and despondency." Opponents of socialized medicine may not agree that the study of history protected Sigerist against errors of judgment; but no one, whether he agrees with Sigerist or not, can deny that Sigerist has had a lasting influence on American medicine not only as a proponent of public medicine, not only as Director of the Institute of the History of Medicine and as Editor of its first periodical, and not only as the real animator in this country of the study of the history of medicine, but most of all by virtue of his essential humanity. These collections of essays present with grace and lucidity the thought of one of our greatest humanitarians, and for this reason especially, we are fortunate, no matter what our procedural beliefs, to have them.

JANE OPPENHEIMER

OTTO RANK. *A Biographical Study based on Notebooks, Letters, Collected Writings, Therapeutic Achievements, and Personal Associations.*

By Jessie Taft. The Julian Press, New York. $6.50. xx + 299 pp. + 1 pl. 1958.

Otto Rank, first a psychoanalyst and continuingly a psychotherapist, was closely associated with Freud during his early professional years, but later became alienated from him. Although the contentions relating to his break with the Master have been taken up in biographies of Freud, this is the first time that Rank himself has been the subject of a major biographical study devoted to his own interests. Rank, like so many members of his professions, was a troubled man, a highly skillful and successful therapeutist, a brilliant and imaginative thinker, and a prolific and fluent author. His biographer, a professor emeritus who specialized in social research and who was vitally interested in psychological problems, was analyzed by him in New York beginning in the autumn of 1926, and remained his disciple, colleague, and friend until Rank's death in 1939.

The book is divided into three major portions. The first, around 55 pages long, discusses Rank's early years, and calls on and quotes liberally from four journals covering the years 1903 to 1905. The next, about ten pages longer, discusses the years between 1906 and 1925 and Rank's association with Freud and dissociation from him; it includes correspondence between Rank and Freud. Slightly more than half the book is comprised by the third section, on the thirteen years of Rank's life after his emancipation from Freud; this deals with his life and work in Paris from 1926 to 1934 and in New York from 1934 to 1939. Something of the character of this section may be surmised from the fact that it presents in whole or in part about 85 letters from Rank to Jessie Taft. A final, brief section of slightly over 25 pages evaluates Rank, one of whose major interests was in artistic creativity, as an artist himself. A list of Rank's published work concludes the volume. There is no index.

The volume is correctly described in its subtitle as a biographical study. It is not quite a full-scale definitive biography. The second sentence of the book states Rank's age in years and months at the time he started his first journal, but the date of his birth is never given, and comparable obliquity of expression characterizes much of the text. The basic differences between Freud's and Rank's therapeutic techniques are not spelled out in simplicity for the novice reader, and no specific attempt is made to summarize or epitomize for the general reader the arguments in favor of Rank's restoration of the will to a position of respectability, and indeed dominance, a position from which other psychologists had deposed it. What we learn of Rank's opinions on the will, on artistic creativity, on the elements, or rather the totality of personality that lie beyond what Rank considered the too-scientific analyses of Freud, we must gather only from the many scattered expressions in regard to them in quotations from Rank's letters and other writings. This study, therefore, will perhaps be of primary interest to those already acquainted with Rank's psychotherapeutics and philosophy. But any readers of this *Quarterly*, who, like myself, were fascinated and illuminated by Ernest Jones' great biography of Freud, will wish to accord this study of Rank its place adjacent to Jones' three volumes on their shelves.

JANE OPPENHEIMER

ECOLOGY AND NATURAL HISTORY

FRONTIERS OF THE SEA. *The Story of Oceanographic Exploration.*

By Robert C. Cowen; introduction by Roger R. Revelle; drawings by Mary S. Cowen. Doubleday & Company, Garden City. $4.95. 307 pp. + 24 pl.; text ill. 1960.

Oceanography is a complex science. Indeed, many scientists who study the oceans feel that it is improper to call oceanography a science. Rather, they say, since "a science" implies "a discipline" and because a broad understanding of the oceans requires an integration of bits of information that by themselves would be called chemical or physical or biological, oceanography does not qualify as "a science." Science or not, oceanography, because of the many facets that are exposed to view, presents a unique challenge to the science writer who would display it in breadth and depth to the lay reader. Robert Cowen accepted the challenge and has produced a book that achieves his objectives as well as most and better than many of the accounts of oceanography for non-oceanographers that have appeared in the last several years.

An introduction by Roger Revelle, the director of the Scripps Institution of Oceanography, a person and a place well known to present-day oceanographers, with pertinent quotations from the Scriptures and the Classics, provides a broad brush sketch of the uniqueness of the Earth as the water planet and a hint of the disciplines of oceanography.

Cowen uses 9 chapters to review most of the major developments in the field. Starting with brief accounts of the ships and people associated with the expeditions of the 1800's, and following with a side excursion in Chapter II to note the Birth of a Planet, Origin of the Seas and Their Basins, and a few other geophysical essentials, the author discusses submarine geology, physical oceanography, marine biology, and ends with a look into the future with chapters on The Promise of Plenty and The New Oceanography.

DAYTON E. CARRITT

THE LOWER ANIMALS. *Living Invertebrates of the World. By Ralph Buchsbaum and Lorus J. Milne in collaboration with Mildred Buchsbaum and Margery Milne. Doubleday & Company, Garden City.* $12.50. 303 pp.; ill. 1960.

Naturalists throughout the world will welcome this handsome, liberally illustrated book about invertebrates. It is generously strewn with black-and-white and colored photography by the world's foremost photographers of invertebrate life: D. P. Wilson, Fritz Goro, and Ralph Buchsbaum. Perhaps the prize photograph in the book is that of the expanded tentacular crowns of two sabellid polychaetes, by Fritz Goro. One must admire the energy of the authors in assembling photographs of invertebrates from widely scattered sources.

One is puzzled, however, as to what sort of audience the book intends to reach. The text tries very hard to be a textbook of invertebrate zoology. Not even the smallest group is omitted, and many technical words are used that are not explained. An excessive amount of space is devoted to the Protozoa, including all the parasitic groups. This information would be of little value to naturalists, and even professional zoologists rarely see parasitic protozoa. It is interesting to note that the Milnes, who wrote the latter half of the text, are able to write in other than their usual palpitating style.

A serious defect of the book is the lack of page numbers on many pages. The pages of colored plates bear no numbers and hence it is time-consuming to locate plates referred to in the text, especially since, because of technical difficulties, many of the colored plates are located far from the text references to them. Few errors have been noted. The table of species numbers, on p. 15, contains the usual understatements; some groups, such as the Mollusca, are badly underestimated. A gorgonian that is not a sea fan is twice referred to as a sea fan. There *are* adult vertebrates with a notochord (p. 286). The photograph of an earthworm (p. 207) is very unclear as to whether it represents one or two earthworms; the clitellum is far from conspicuous to the inexperienced eye. However, it is ungracious to pick small faults with such a beautiful book.

L. H. HYMAN

DRAGONFLIES. *The New Naturalist. By Philip S. Corbet, Cynthia Longfield, and N. W. Moore. Collins, London.* 42s. xxi + 260 pp. + 69 pl.; text ill. 1960.

In their aim "to interest the general reader in the wild life of Britain by recapturing the inquiring spirit of the old naturalist," the editors of *The New Naturalist* series have enlisted the collaboration of three eminently qualified scientists to essay the Odonata. Corbet, Moore, and Miss Longfield have acquitted themselves admirably in executing a standard reference which will serve as an ecological companion piece to the 1917 classic *Biology of the Odonata* by R. J. Tillyard. With its emphasis on life history, ecology, and ethology, this volume could not have been written before the middle of the twentieth century inasmuch as it has become fashion largely since the second world war, the editors admonish, "to study insects as animals, and to promote fundamental research into their place in nature."

The aforementioned emphases of the book make it a remarkable volume. This accolade is supported additionally by several factors. First, its decidedly modern ecology and ethology is tempered by occasional anecdotes and less frequent anthropomorphisms, remnants of that delightful peculiarity of the literary naturalist. Secondly, in retaining each author's style, the editors have allowed a personal flavor to each chapter. But the most notable attribute of the book is the advising of the excellence of dragonflies as a tool of ecological and ethological investigation. Aptly termed the "birdwatcher's insect" because of their size, activity, and color, the Odonata are well-fitted for behavioral studies on territoriality, sexual display and courtship, and dispersal, and for ecological studies on habitat selection, population dynamics, and seasonal regulation.

Many of the ideas developed in the text are theoretical and have been derived from earlier papers by the authors. This is especially true of the sections on ethology by Moore and on ecology by Corbet. Miss Longfield's lucid and almost non-technical discussion of systematics and distribution permit easy acquaintance with the British species and are prerequisite to the discussions by her collaborators.

Diapause, a relatively unstudied problem in the Odonata, is proposed by Corbet to be a major factor in seasonal regulation. He postulates that species which are synchronized to undergo final metamorphosis in late spring or early summer undergo diapause in the ultimate instar, whereas those emerging later in the year and not demonstrating any synchronization diapause in the egg or some instar other than the last, if at all. He also proposes that photoperiod regulates the final synchronization to enable the majority of the population to emerge within a few days: this hypothesis is proving fruitful of more investigation. Corbet has also contributed the several excellent chapters on ecological life history, containing many provocative ideas, and the summary of fossil history.

In his discussion of the behavior of the adult dragonfly, Moore argues under a much less rigid concept of territoriality than in his earlier papers on the subject. Previously he denied the occurrence of territorial behavior in the several species now listed as being territorial. He rightly points out, however, that in these forms aggressive display is not so easily

distinguishable from sexual behavior as in the American genera *Plathemis* and *Perithemis*. A summary statement on the critical studies of Christiane Bucholtz on releasers in damselfly behavior is evidence of the Tinbergen-Lorenz orientation of the author. The discussion on dispersal, acknowledged by the author to be deficient in supportive evidence, compensates for its weakness by speculation and challenge to study this almost untouched field. The discussion on economic importance interjects several interesting comments on the aesthetic nature of the Odonata and contains a well-deserved "plug" for the Nature Conservancy organization of which Moore is a regional officer.

More than passing mention must be allowed the superb illustrations, well worth the price of the book alone. Thirty-seven of the British species, photographed by Mr. Sam Beaufoy, have been reproduced exquisitely in 54 color plates. The black and white photographs include an excellent account of the final ecdysis of *Anax imperator*, by Mr. Beaufoy. Some of the text illustrations suffer by comparison with those in the appended and reprinted key to larvae, by A. E. Gardner. The appendices also include a distribution map of each species, methods for preserving color, and for marking larvae and adults for population studies.

This is a welcome and unique addition to the literature on the Odonata. Written lucidly and invitingly, it is a challenging reminder of British skill in exposition and perception.

EDWARD J. KORMONDY

THE RELATIONSHIP OF THE PEARY AND BARREN GROUND CARIBOU. *Arctic Institute of North America. Technical Paper No. 4.*

By T. H. Manning. Arctic Institute of North America, Montreal. $2.00 (paper). 52 pp.; ill. 1960.

EVOLUTION

EVOLUTION: *Process and Product. Revised Edition of* A TEXTBOOK OF EVOLUTION.

By Edward O. Dodson. Rheinhold Publishing Company, New York; Chapman & Hall, London. $6.90. xvi + 352 pp.; ill. 1960.

This is a revised edition of Dodson's *A Textbook of Evolution* (W. B. Saunders, 1952), which has acquired a new title, a new publisher, and many improvements in the text and in the illustrations. The revision has become necessary because of the immense progress in the study of evolution in recent years, this study being " . . . at at present highly fashionable among biologists." The job is so well done that the author's hope that the book " . . . will prove even more useful than its predecessor" is surely justified.

The plan of the book remains about the same as in the first edition. More than half of the volume is taken up by the presentation of Evidences of Evolution, and by a review of the phylogeny of the plant and animal kingdoms. The mechanisms of the evolutionary changes are by no means neglected, however. Quantitative aspects of evolution, gene frequencies, selection and mutation pressures, population size, genetic drift, allometry, and much else is discussed briefly, but clearly and adequately for the purposes of a college textbook, as is the Watson-Crick model of the structure of DNA, and some recent biochemical and immunological discoveries.

The author is clearly skeptical concerning the validity of the modern neo-Darwinian, or biological, theory of evolution. His sympathies seem to lie with R. Goldschmidt's view that ascribes evolutionary importance to so-called "systemic" mutations. This is certainly a legitimate minority opinion and, very judiciously, the author does not permit it to bias his presentation, which is fair and accurate throughout.

LEE EHRMAN

MISSISSIPPIAN AND DEVONIAN PALEONTOLOGY AND STRATIGRAPHY, QUARTZ SPRING AREA, INYO COUNTY, CALIFORNIA. *Univ. Calif. Publ. Geol. Sci., Vol. 38, No. 2.*

By R. L. Langenheim, Jr. and Herbert Tischler. University of California Press, Berkeley and Los Angeles. $1.25 (paper). Pp. iv + 89–147 + pl. 14–15; text ill. 1960.

STRATIGRAPHY AND PALEONTOLOGY OF THE PERMIAN NOSONI AND DEKKAS FORMATIONS (*Bollibokka Group*). *Univ. Calif. Geol. Sci., Vol. 36, No. 5.*

By Alan H. Coogan. University of California Press, Berkeley and Los Angeles. $1.75 (paper). Pp. iv. + 243–302 + pl. 22–27; text ill. 1960.

THE LIASSIC AMMONITE ZONES AND SUBZONES OF THE NORTH-WEST EUROPEAN PROVINCE. *Bull. Brit. Museum Nat. Hist. and Geol., Vol. 4, No. 10.*

By W. T. Dean, D. T. Donovan, and M. K. Howarth. The British Museum (Natural History), London. 35s. (paper). Pp. ii + 438–505 + pl. 63–75. 1961.

LATE PLIOCENE FLORAS EAST OF THE SIERRA NEVADA. *Univ. Calif. Geol. Sci., Vol., 39, No. 1.*

By Daniel I. Axelrod and William S. Ting. University of California Press, Berkeley and Los Angeles. $2.50 (paper). iv + 68 pp. + 24 pl.; text ill. 1960.

GENETICS AND CYTOLOGY

VERERBUNGSLEHRE.

By Arne Müntzing; translated by Diter von Wettstein. Gustav Fischer Verlag, Stuttgart. DM 42.-. xii + 303 pp.; ill. 1960.

This translation of Müntzing's book into German provides the first opportunity many biologists have had to acquaint themselves with this, the standard Swedish textbook of genetics. It proves to be complete, well written, amply illustrated, and generously supplied with examples from the fields of plant, animal, and human inheritance. Though originally published in 1953 and not greatly revised for translation, some changes have been made, and the result is a reasonably up-to-date text. The chromosome number of man, for example, is given as 46 rather than 48.

The book does not follow the conventional order of presentation, but begins with a description of the chromosomes and their behavior during mitosis and meiosis. The laws of heredity are then shown to be the logical consequence of the chromosomes' being the carriers of the genes. Considering Müntzing's enviable record as a plant cytogeneticist with practical interests, it is no surprise that more than the usual emphasis is placed on cytogenetics and that the chapters on polyploidy and plant breeding are especially strong. Understandably, very many examples have been taken from Swedish work. An excellent glossary is appended. Unlike the leading American textbooks, this one does not deal with statistical considerations, and no questions or problems are supplied at the ends of chapters. German reviewers have been laudatory and have agreed that the book fills a need in their country.

E. R. Sears

Principles of Genetics.

By Eldon J. Gardner. John Wiley & Sons, New York and London. $7.50. viii + 366 pp.; ill. 1960.

The choice of textbooks in genetics is large, and the field has widened recently by the accession of several new books, published both in America and in England. Among these, the book by Eldon J. Gardner is one of the simplest. One hopes that its elementary nature will bring genetics to the level of understanding of a medium-bright college student. It is probably adequate and suitable for a one-semester course.

The presentation follows classical lines, opening with Mendel's experiments, introducing chromosomes in Chapter 4, sex-linkage in Chapter 7, mutation in Chapter 12, physiological and population genetics in Chapters 15 and 16, respectively, and ending with a brief discussion of the applications of genetics in agriculture, and with 4 concluding pages concerning human genetics (Chapter 18). The text is brightened by good illustrations, both diagrams and photographs, by artistic portraits of some eminent geneticists, and by human interest stories concerning their lives and activities.

Lee Ehrman

Advances in Genetics. *Volume IX.*

Edited by M. Demerec. Academic Press, New York. $8.80. viii + 294 pp.; ill. 1958.

The latest volume in this standard review series in the field of genetics contains 6 reviews: The population genetics of *Drosophila robusta* (Hampton L. Carson); The genetics of basidiomycetes (Haig P. Papazian); Genetic analysis based on mitotic recombination (G. Pontecorvo & Etta Käfer); An 8-chromosome map of *Aspergillus nidulans* (Etta Käfer); The inviability, weakness, and sterility of interspecific hybrids (G. Ledyard Stebbins); and The mechanism of sex determination in dioecious flowering plants (M. Westergaard). These are all solid, meaty reviews, designed to cover a subject thoroughly rather than as readable general essays. They require perusal, not skimming.

The review of *Drosophila robusta* population genetics and the reviews of the nature of hybrid sterility and inviability barriers between species and of sex determination in dioecious plants will have particular interest for students of evolutionary processes. The two last-named reviews have been greatly needed and will unquestionably remain the standard reference works on their respective subjects for quite a few years to come. It is particularly gratifying that they have been so carefully prepared, and by authorities second to none. The review of the population genetics of a single *Drosophila* species has merit of another kind, since it is illustrative of the insight that arises from an exhaustive treatment of all aspects—cytological, geographical, ecological, and morphological—of the genetic variability of a selected species.

Microbial geneticists will be pleased to have the three remaining reviews available for reference. The survey of the complexities of basidiomycete genetics covers the mating system, formation of heterokaryons, mutation, segregation, cytoplasmic autonomy, and nucleocytoplasmic relations. The introductory discussion of genetic analysis based on mitotic recombination in fungi should make familiar to many geneticists who are acquainted only with meiotic recombination the nature of the fruitful supplement, or alternative (the sole method in forms lacking sexual reproduction) to such studies, available in fungi through the occurrence of haploidization and mitotic recombination. The resulting linkage maps, as might be expected, reveal the same linear order of genes, but the distribution of recombination is quite different, being especially concentrated near the centromere and limited to a meiotic linkage distance of about 28 units from it. The remaining review in this volume will be principally, if not entirely, of interest to workers with *Aspergillus* and similar fungi. It establishes, by means of an analysis of mitotic haploidization, the existence of 8 linkage groups in *Aspergillus nidul-* ss. Most of the 40 available

markers fall into 4 linkage groups, the other 4 being as yet sparsely represented. Good chromosome maps are now presented for linkage groups I and II.

BENTLEY GLASS

GENERAL CYTOLOGY. *Third Edition.*

By E. D. P. De Robertis, W. W. Nowinski, and Francisco A. Saez. W. B. Saunders Company, Philadelphia and London. $10.00. xvi + 555 pp.; ill. 1960.

The earlier editions of this book, although widely used, have really never been considered satisfactory. The latest edition has been considerably revised, and an attempt has been made to bring the text into line with the material and information with which a modern cytologist is expected to be conversant. The sections dealing with the chemical organization and ultrastructure of cells are particularly well presented. One might have liked a more extensive consideration of the cell in development to complement the section on cytogenetics, but this does not detract from the way in which the general subject of cytology was handled. This book will prove to be a particularly useful textbook for the beginning course in cytology.

R. R. COWDEN

THE CELL NUCLEUS. *Proceedings of an informal meeting held at the Department of Radiotherapeutics, University of Cambridge, 31 Aug.–1 Sept., 1959, by The Faraday Society.*

Chairman: J. S. Mitchell. Academic Press, New York; Butterworths, London, Toronto, Sydney, Wellington, and Durban. $11.00. xiv + 269 pp.; ill. 1960.

The 37-brief reports of current research contained in this symposium volume scan the nucleolus, the structure and biosynthesis of DNA, the DNA content of some viruses, chromosomes, and nuclei, and some enzymes associated with nuclei. Brief discussions of some of the papers are included, as well as an index. The volume gives to specialists a scattered account of some current research trends in the delineation of the structure and function of the nucleus and its components.

PHILIP E. HARTMAN

GENERAL AND SYSTEMATIC BOTANY

UMBELLIFERAE OF JAPAN. *Univ. Calif. Publ. Botan., Vol. 30, No. 1.*

By Minosuke Hiroe and Lincoln Constance. University of California Press, Berkeley and Los Angeles. $2.75 (paper). ii + 144 pp.; ill. 1958.

M. Hiroe, who has collected widely in Nippon and has published three papers on the Umbelliferae of the islands, and Lincoln Constance, whose publications on the American Umbelliferae, many in collaboration with Mathias, are well known, joined forces to produce what they hoped might be "a useful framework for subsequent detailed studies" of the anatomy, cytology, and genecology of this plant family in Japan. This admirable illustrated account of the 76 recognized species bears witness of their success. It is a strictly taxonomic account, without reference to the biology or economy of the species. There is no mention of esculent species such as skirrets (*Sium sisarum*), noticed by Thunberg when he visited Japan in 1776—Thunberg also noted fennel, anise, parsley, dill, and carrots "which here are of a colour very little inclined to yellow"—nor are there remarks in regard to habitat or frequency, except in the most general terms.

Though conservative in the treatment of genera and species, following the earlier conclusions on North American Umbelliferae drawn by Constance and Mathias, the present account, as the authors point out, is essentially in accord with the recent writings of Japanese botanists. Japanese botanists have recognized from 31 to 33 genera, as against 29 here, and anywhere from 56 to 90 species, as compared with 76 species in this treatment. Only Hara, up to now, has studied the Japanese "umbels" with close attention to cognate American species. Hiroe and Constance present keys, descriptions, indication of types, synonymy, and citation of collections, for each species. All 76 species but the carrot are illustrated, mostly with both habit and carpel drawing, the majority of which are the work of Mr. G. Nakai, an artist of consummate skill.

The close affinity of the Japanese and North American floras has often been commented on, following the now classic paper of Asa Gray (1859). About half the Japanese Umbelliferae also occur in North America; 3 genera, *Glehnia*, *Oenanthe*, and *Bupleurum*, are represented in western North America but are absent from eastern North America; and, in the alternate pattern, only *Cryptotaenia* occurs in Japan and eastern North America, but not on the western edge of the North American continent. The predominant floristic connections are with Eurasia, but there are two endemic genera in Japan, *Chamaele* and *Apodicarpum*, both monotypic. One fourth of the "umbels" of Japan are endemic.

A comparison of Japanese and Texan Umbelliferae is of interest, since both umbelfloras have been under study by Constance (*Q. R. B.*, 29: 66. 1954), and we therefore have a useful frame of reference. Japan, with a little more than half the area of Texas, possesses about three-fourths as many genera as that great state, or 29 genera vs. 42 Texan genera. *Angelica*,

the largest genus in Japan (23 species), though present in North America in the number of 22 species, is wholly absent from Texas. Many genera, on the other hand, are common to the two climatically unlike regions; and 3 species, *Torilis japonica*, *Cryptotaenia canadensis*, and *Sium suave*, occur in both Japan and Texas. In the 1951 account of Texan Umbelliferae *Torilis japonica* was considered a "Mediterranean weed"; *Sium suave*, wholly American. *Centella erecta*, of Texas and *C. asiatica*, of Japan, are geminate species. Incidentally, the statements in the Japanese account concerning type localities are a valuable addition over the Texan, but the notes on Thunberg's localities in the former may be given greater precision by reference to his *Travels*. This work, first published in Swedish and translated into English (rather defectively) into French (too briefly), and into German, are too little known, considering their fullness and charm. To learn more about Papenberg and Kosido, mentioned under *Peucedanum japonicum*, for example, read Thunberg's *Travels* (3: 60–61. 1795) on those islands.

JOSEPH EWAN

THE GENUS *Hesperolinon* (LINACEAE). *Univ. Calif. Publ. Botan., Vol. 32, No. 4.*

By Helen K. Sharsmith. University of California Press, Berkeley and Los Angeles. $1.50 (paper). Pp. iv + 235–314; ill. 1961.

PLANT MORPHOLOGY AND DEVELOPMENT

SCHIZOPHYZEEN. *Handbuch der Pflanzenanatomie. Abteilung: Spezieller Teil. Band VI, Teil 1.*

By Lothar Geitler. Gebrüder Borntraeger, Berlin. DM 57.50. viii + 131 pp.; ill. 1960.

For the phycologist and for those interested in broad problems of cellular organization, the publication of the second edition of Lothar Geitler's volume on the blue-green algae is an important event. The 131 pages of this deceptively small volume are packed with information and excellent illustrations, many of them new and original. This information is both historical and current in scope. The review of our past and present concepts of cellular organization is especially valuable. The major topics discussed are the organization of the protoplast, external morphology (including cell form and size and membrane structure), the life cycle (including growth, cell division, and special reproductive structures), and a final comparative treatment of organization in unicellular, colonial, and filamentous types. Treatment of taxonomic problems obviously is not considered in this volume of the *Handbuch der Pflanzenanatomie*, but the morphological material summarized certainly forms an important basis which must be reckoned with in the taxonomy of these plants. An excellent bibliography and index complete the volume. The author has made a most important addition to phycology in a field to which he has contributed much original work.

HAROLD C. BOLD

THE PLANT CELL-WALL. *Handbuch der Pflanzenanatomie. Abteilung Cytologie. Band III, Teil 4.*

By Pieter Roelofsen. Gebrüder Borntraeger, Berlin. DM 135.-. viii + 335 pp. + 68 pl.; text ill. 1959.

This book is a new edition of the volume on the plant cell wall in the *Handbuch*. Research on cell walls has been extremely productive since the publication of the first edition (Van Wisselingh, 1925). The development of electron microscopy provided a particularly strong impetus to this research. To keep the review within reasonable limits the author chose to stress the present status of knowledge on cell walls.

The contents are divided into a part on cell wall constituents (101 pp.) and another on cell wall structure (204 pp.). Over 750 references are cited, and 215 illustrations of high quality—mostly electron micrographs—accompany the text. Cell walls of all groups of plants are considered. There are separate indices for the authors, the subject matter, and the botanical and zoological names.

The first part gives a comprehensive review of the occurrence, physical structure, biosynthesis, and identification of cell wall constituents. Explanations of terms and methods, and evaluations of the significance and limitations of both, enhance the usefulness of the presentation. One should also commend the unbiased appraisal of concepts and the clear exposition of the author's own views. The botanist will find the reviews on chitin, hemicelluloses, lignin, and lipoidal substances especially helpful. Brief treatments of miscellaneous substances that may accompany the basic wall constituents round out the discussion. This part is a highly serviceable reference work for botanists and biochemists.

The treatment is not of equal accuracy and discernment throughout. In defining the micelle, for example, the author states that he follows Frey-Wyssling in using the term for either crystalline or non-crystalline particles. Actually, Frey-Wyssling speaks of the micelle as a region in the cellulose framework and not as a particle. Another example is the inadequate treatment of the controversial question of delignification. One also wonders about the basis for the statement that decomposition of lignin occurs when xylem

vessels are perforated; the wall parts to be perforated do not become lignified.

The topic of cell wall structure begins with methods of study, and again terms are explained. An instructive set of diagrams illustrates the orientation of microfibrils in different kinds of cells and the relation of this orientation to the character of birefringence of the walls. The structure and growth of the primary wall are given due consideration. Of especial interest is the analysis of the multi-net method of wall growth (a concept introduced by the author) and the attempt to relate this type of growth to the direction and distribution of tensions in the wall. An imaginative hypothesis on the displacement of cell wall material in relation to the distribution of the turgor pressure is developed. The section on the cell plate is disappointingly brief. In this connection, the reference to the separating cells in Plate 5 as dividing cells is confusing.

The section of the primary wall is begun auspiciously with a definition of primary and secondary walls. Commendably, the author attempts to reconcile the discrepancies in the delimitations of these walls as based on light microscopy on the one hand, and on electron microscopy on the other. He also knows how difficult it is to be certain of the state of growth of a wall—a feature involved in the classification—that has been prepared for electron microscopy. This sound attitude does not persist, however. Later, reference to secondary walls is made without a qualification, even when thin-walled root hairs and parenchyma cells are discussed. Sometimes one is not certain whether the primary or the secondary wall is being described. Thus, the collenchyma wall is described under the general heading of Structure of Cellulose in Some of the Best Known Secondary Walls; at the same time an implication is made that secondary walls of these cells have not been studied by the electron microscope.

The comparative analysis of the three layers commonly found in xylem fibers and tracheids is highly informative, and the critical review of the concept of a tertiary wall is helpful. The secondary wall of the cotton hair is taken up in detail, and an instructive structural diagram of this wall is newly devised. The discussion of the secondary wall is strongest in the area of electron microscopy. Information obtained with the light microscope is less adequately treated and, as I. W. Bailey (*Science*, 132: 1245. 1960) has pointed out, the remarkable correlation between the findings at the two levels of microscopy is not brought out. The cutinized and suberized walls are discussed comprehensively, with emphasis on the character of their birefringence. A review of wall structure in algae and fungi concludes the exposition.

A few remarks should be made about the organization and style of the book. It is to be regretted that no summaries are given; that the numbering of the text figures is intermixed with that of the figures in the plates; that the legends furnish so little information. The text, translated from the Dutch, would have been improved by additional editing. Germanic sentence structure is frequently encountered.

Despite the imperfections, botanists should be grateful to the author for bringing together the information on the chemical and physical structure of the cell wall, for clarifying many concepts, and for pointing out problems in need of further study. Unfortunately, the price of the book is prohibitive.

KATHERINE ESAU

PHLOEM STRUCTURE IN *Pyrus Communis* L. AND ITS SEASONAL CHANGES. *Univ. Calif. Publ. Botan., Vol. 32, No. 2.*

By Ray Franklin Evert. University of California Press, Berkeley, and Los Angeles. $1.50 (paper). Pp. iv + 127–169 + pl. 15–25; text ill. 1960.

ECONOMIC BOTANY

AN INTRODUCTION TO INDUSTRIAL MYCOLOGY. *Fifth Edition.*

By George Smith; foreword by Harold Raistrick. Edward Arnold, London; [St. Martin's Press, New York]. $10.00. xvi + 399 pp.; ill. 1960.

Traditionally, the English make social introductions with great propriety. This ability has carried over into scientific areas as well: the present volume is a good example. Many people have become acquainted with molds from previous editions of this book without extensive prior preparation in botany or mycology. Many more will undoubtedly do likewise in the future, for no other textbook sets forth this comprehensive subject in such simple language.

The author has not altered the format of the book in the present edition, the only change in this regard being the separation of author and subject indices. Emphasis remains on brief descriptions and excellent illustrations of the genera of molds of major industrial significance. The first 10 chapters are devoted to this area, and judiciously selected references are included to enable a more thorough study of any specificities necessary. The new edition contains many minor changes and additions. In fact, it is about ten per cent larger in number of pages and in literature cited than the fourth edition, of 1954 (*Q.R.B.*, 30: 175. 1955); and, on the basis of their relative sizes, one must expend fifty per cent more effort to become acquainted with *Industrial Mycology* now than was the case

fifteen years ago, when the third edition appeared in 1946 (*Q.R.B.*, 21: 382. 1946). It would appear that Smith is fighting a losing battle in his attempt to retain both simplicity and completeness in a single volume, but he is making a valiant attempt to accomplish these objectives.

Chapters XI through XVI contain information of general interest. Although the sections on taxonomy are excellent, some of these chapters could have undergone more extensive modification. For example, Chapter XV, on Industrial Uses of Fungi, contains very poor discussions of the present status of many fermentations, such as the production and uses of fungal enzymes and citric acid fermentation. Microbial insecticides and the gibberellins are not mentioned. This chapter might be replaced in future editions with a list of appropriate texts and reviews. Several other chapters could probably be expanded somewhat, for example, to include the slide method of Taschdjian (*Mycologia*, 46: 681–683. 1954) in Chapter XI, on Laboratory Equipment and Technique. After the emendations of four previous editions there is little lack of clarity, but it was noted that in Chapter XIII, on Maintenance of a Culture Collection, mention was made of preservation under sterile mineral oil, yet no directions are given regarding the difficult and critical process whereby mineral oil may be sterilized.

PAUL A. HARTMAN

GENERAL AND SYSTEMATIC ZOOLOGY

WARD AND WHIPPLE'S FRESH-WATER BIOLOGY. *Second Edition.*

Edited by W. T. Edmondson. John Wiley & Sons, New York; Chapman & Hall, London. $34.50. xxii + 1248 pp.; ill. 1959.

The present volume represents an extensive revision of the original edition, published in 1918. It contains contributions from 51 authors each of whom is an authority on the particular group of organisms treated. There are terse general discussions and classifications in addition to keys to freshwater plants and invertebrate animals. The treatise includes chapters on fungi, bryophytes, polychaetes, and tardigrades which were lacking in the first edition. Some chapters of the book have, of course, been more extensively revised and enlarged, such as those concerned with the protista (interpreted to include bacteria, algae, fungi, and protozoa), nematodes, copepods, malacostraca, and vascular plants. Certain sections, such as keys to internal parasites and vertebrates, which were included in the 1918 edition, have been omitted, but their exclusion does not detract from the value of the book. The discussion of internal parasites does not logically belong in such a treatise, and there are adequate keys available elsewhere for the identification of aquatic vertebrates.

Despite the excellence of the original edition, it had become outmoded. In the new edition the material has been brought up to date and enlarged. The new format and the inclusion of many additional genera and species add greatly to its usefulness. The keys are profusely illustrated with good line drawings, some of which are reproduced from the first edition, but many are new and represent the diagnostic features very well.

With respect to the chapter on the ciliates, Noland has done an excellent service in bringing together invaluable material from such classical sources as Kahl and Penard. A number of the chapters retain helpful explanations as to methods of collecting and preserving the organisms discussed.

This book has great potential worth for the interested beginning student as well as for the professional biologist. Its scope is broad. On the whole, the keys are well planned and workable. There is emphasis upon structures of use in keying and identification, many of which have little or no importance in terms of taxonomy. However, modern classifications are presented, or specific references to them are given, so that one can find the current taxonomic position of any given genus which is keyed.

EDITH MORTENSEN

INERTEBRATE ZOOLOGY.

By Victor Schechter. Prentice-Hall, Englewood Cliffs. $6.95. xiv + 530 pp.; ill. 1959.

It would be difficult to imagine a more fundamentally unsatisfactory book than Schechter's *Invertebrate Zoology*. It is far too elementary for advanced classes, and while written at the elementary level, presents too much detail for a beginning zoology class. There is also an indication that the book was not carefully proofread, since a figure of a copepod appears opposite p. 69 as figure 2.31 labeled "*Polystomella*, mature form," while a plate of *Polystomella* appears opposite p. 374 labeled "*Calcoalanus*, (*Calanus*), an important marine copepod." The text is profusely illustrated with line drawings and photographs, but these seem more in keeping with an elementary text than with an advanced one. While the work cannot be recommended as a textbook for a course of study, it might be useful as a supplement to the general zoology textbook when the Invertebrata are under consideration.

R. R. COWDEN

REVISION OF THE WORLD SPECIES OF *Aplysia* (GASTROPODA, OPISTHROBRANCHIA). *Bull. Brit. Museum Zool., Vol. 5, No. 10.*

By N. B. Eales. The British Museum (Natural History), London. 45s. (paper). iv + pp. 270–404; ill. 1960.

This work deals with one of the best-known but least understood groups of marine organisms, the sea hares, so called by Pliny because he thought that he could detect a resemblance between the pose of their tentacles and the ears of the jack-rabbit. They are popularly known as "ink fish" because of the purple fluid secreted by the mucus gland. This brings to mind the first experiment of my scientific career, when I sought to use this secretion as a writing fluid. As might be expected from a product of the mucus gland, it cannot be used in a fountain pen until it has been strained through filter paper, but after this has been done it makes a very acceptable ink which even today shows no signs of fading after more than half a century.

N. B. Eales has recognized 35 legitimate species of the two genera of the subfamily Aplysiinae, as well as 15 more of doubtful validity which she has distributed among 5 subgenera. Most of these are to be found in tropical or subtropical waters, but one species does occur in the Arctic.

Sea hares are well known because they are so abundant; they are poorly understood because they have no features which the ordinary beach collector is likely to find attractive. It is a simple matter to prepare a molluscan shell for preservation in a show case and, as most molluscan species are discriminated by conchological rather than anatomical features, the shell in most instances suffices for taxonomic purposes. But in the genus *Aplysia* the shell is greatly reduced, affording protection to the liver only, and it is perfectly concealed by the epipodia which are reflected over the shell and fused. Therefore the taxonomy must be based upon the soft parts, and no preservative is known which does not shrink and distort the tissues and bleach their pigmentation. Consequently it is difficult to make satisfactory studies except on fresh material.

Another reason why the study of this group has not appealed to the scholars of past generations is indicated by the specific name of the generitype—*depilans*. This name was suggested to Linnaeus by the ancient belief that contact with these organisms caused alopecia. The contact, it was supposed, did not even need to be physical; Aldrovandus stated that a pedestrian need only touch one of these creatures with a walking cane in order to be affected. Nobody wants a bald head, and the possibility of acquiring one was an effective deterrent to anyone who might have been contemplating an anatomical investigation of *Aplysia*. Linnaeus, of course, did not share this belief. He was a doctor of medicine, but he may have accepted the name because it had so long been in use.

The present treatise is probably the best that has ever been compiled on the group. Nevertheless, it is not perfect. Its few errors, however, are mostly not of a taxonomic nature. One error is the statement that Pilsbry (whose monograph in the *Manual of Conchology* is second best) placed the genus *Aplysia* in the Tethyidae, and the genus *Tethys* in the Aplysiidae. This statement is not fair to Pilsbry; what he actually did was to apply the law of priority to these names. Linnaeus had called this genus of the Tectibranchiata *Tethys* in his 10th edition, published in 1758. In the 12th edition, of 1767, he transferred this name to the nudibranch genus which Bohadsch had called *Fimbria* in 1861, and to the tectibranch he gave the names *Laplysia* and *Aplysia*. The former is generally considered a lapsus calami, although some have interpreted the initial to represent the French definite article. The first reviser was Gmelin, who prepared the 13th edition, in which he used *Aplysia* for the tectibranch and *Tethys* for the nudibranch. Such usage, of course, is not consistent with the rules, but it was accepted by practically every writer until Pilsbry restored the original meaning to *Tethys*. It was not until 1954 that the International Commission on Zoological Nomenclature validated *Aplysia*, as used in the 12th edition, by suppressing the names *Tethys* Linnaeus 1758 and *Fimbria* Bohadsch 1761. The pointlessness of criticizing Pilsbry's action, taken a half century more or less before the Commission acted on this name, is obvious. Eales has also erred in locating Cedros Island in the Gulf of California.

Finally (and really the only serious error), the illustration that purports to represent the shell of *Aplysia californica* is almost certainly something else. The shell of *Aplysia californica* retains the dextral helicospiral coiling of its ancestors, and although it has lost most of its calcareous content, the corneous material which remains is quite rigid. Surrounding it is a flounce of similar corneous material which is not rigid at all. As the shells dry out, the flounce tends to flake away, as if the two parts of the shell had different shrinkage rates.

In the preparation of this publication 468 specimens were examined and 257 bibliographic references consulted. There are 196 specific and subspecific names in the index, most of which, of course, are synonymous.

The family Aplysiidae, in addition to the genera *Aplysia* and *Syphonota*, contains 8 others. If the author could monograph all of these, systematic malacologists would owe her a great deal of gratitude.

JOSIAH L. BAILY, JR.

CHECK LIST OF PLANT AND SOIL NEMATODES. *A Nomenclatorial Compilation.*

By 'Armen C. Tarjan. University of Florida Press, Gainesville. $7.50. xiv + 200 pp. 1960.

This check-list provides useful and recent information on the nomenclature and taxonomy of the plant and soil nematodes. It is based largely on Stiles and Hassall's *Index-Catalogue* (1920), Baylis and Daubney's *Synopsis* (1926), Goodey's *Plant Parasitic Nematodes* (1933), and Goodey's *Soil and Freshwater Nematodes* (1951). The specific names are listed alphabetically and are followed by names of subspecies, varieties, forms, etc., with a number which corresponds to a reference in the literature at the end of the book. Entries under the specific name are listed in chronological order. Synonyms and homonyms are preceded by a number designating the reference in which the determinations were made.

HORACE W. STUNKARD

THE GENUS LAELAPS. *With a Review of the Laelaptinae and a New Subfamily Alphalaelaptinae (Acarina: Laelaptidae). Univ. Calif. Publ. Entomol., Vol. 16, No. 6.*

By Vernon J. Tipton. University of California Press, Berkeley and Los Angeles. $2.50 (paper). vi + pp. 233–327 + pl. 22–47. 1960.

As modern systematic techniques have become more widely used, there has been a refreshing shift from faunal treatments to monographic treatment of various systematic categories. This study of the genus *Laelaps* is such a monographic treatment. Even though our present knowledge of the genus *Laelaps* is quite fragmentary, this study clearly demonstrates the superiority of the monographic treatment over the faunal treatment.

In defining the genus *Laelaps*, the subfamily to which the genus belongs is characterized and compared with other subfamilies of the family. As a result of this comparison, a new subfamily, the Alphalaelaptinae, is recognized. Once the subfamily Laelaptinae has been defined in terms of exclusion, that is by ruling out the other subfamilies, it is then further described in terms of inclusion. Each of the genera currently assigned in the subfamily is diagnosed, and a key is provided for the recognition of the various genera of the subfamily. This key is developed in terms of morphological features of the female. The fact that differentiation of the genera on the basis of other stages, such as males, nymphs, and larvae, has not been made is merely confirmation of the meager state of knowledge available concerning the genus *Laelaps*. By discussion of all of the genera of the subfamily Laelaptinae, the genus *Laelaps* is, of course, defined in terms of exclusion. One of the very valuable characteristics of this paper is that a list of included species is given for each of the genera that are discussed. The genus *Laelaps* is next defined in terms of inclusion by listing those properties of the species included in the genus that are fairly common to all and then by considering all of the included species. Fifty-one species are assigned to the genus as here interpreted. Of these, a key to 44 species is given, while the other 7 species came to the attention of the author too late for inclusion.

In consideration of the species of the genus *Laelaps*, morphological features, comparison with closely related species, and collection data are included. The author has made extensive attempts to obtain material of all included species, but he was unable to obtain material of many species because museum materials are not properly available. There is little doubt that this summary of the species of the genus *Laelaps* will result in a much more careful and thorough study of the genus so that the monograph will rapidly go out of date.

Certain other very meritorious features of this monograph should be listed. A list of species formerly assigned to the genus is included. A list of hosts and members of the genus that parasitize these hosts is provided. The bibliography is extensive and quite adequate. The monograph ends with excellent illustrations of the features of the genus in 26 plates.

G. W. WHARTON

THE STONEFLIES (PLECOPTERA) OF CALIFORNIA. *Bull. Calif. Insect Survey, Vol. 6, No. 6.*

By Stanley G. Jewett. University of California Press, Berkeley and Los Angeles. $1.25 (paper). iv + pp. 125–177; ill. 1960.

MICROLEPIDOPTERA FROM THE SOLOMON ISLANDS. *Additional Records and Descriptions of Microlepidoptera Collected in the Solomon Islands by the Rennell Island Expedition 1953–54. Bull. Brit. Museum Nat. Hist., Entomol., Vol. 10, No. 4.*

By J. D. Bradley. The British Museum (Natural History), London. 30s. (paper). ii + pp. 114–168 + pl. 5–19; text ill. 1961.

PRIMATES. *Comparative Anatomy and Taxonomy. IV. Cebidae, Part A. A Monograph.*

By W. C. Osman Hill. Interscience Publishers, New York; Edinburgh University Press, Edinburgh. $27.75. xxiv + 523 pp. + 36 pl; text ill. 1960.

This is the fourth of a series of monographs about Primates by W. C. Osman Hill. The three earlier volumes were devoted, respectively, to the lemurs (1953) (*Q.R.B.*, 29: 337. 1954), the tarsiers (1955) (*Q.R.B.*, 31: 310. 1956), and the marmosets and tamarins (1957). The present volume deals with the family Cebidae, which comprises most of the platyrrhine or New World monkeys. However, only the subfamilies Callicebinae, Aotinae, Pitheciinae, and Cebinae are considered here. The author intends to

deal with the subfamilies Alouattinae and Atelinae in a separate monograph.

Like the earlier volumes, the present one covers a broad spectrum, dealing with the anatomy, development, behavior, and taxonomy of the included genera. It is by no means a mere compilation of the literature, for it contains a large number of original and unpublished observations by the author. It is well illustrated and has a large and useful list of references to the literature.

This monograph, like the previous ones of this series, represents the most complete and most useful reference book available for the area which it covers. It contains not only most of the existing available data about *Callicebus*, *Aotes*, *Pithecia*, *Chiropotes*, *Saimiri*, and *Cebus*, but also reveals, indirectly, the gaps in our present knowledge of these genera. These gaps, in fact, are so considerable that only one genus, *Cebus*, is reasonably well known. Considering the magnitude of the task which he has undertaken, one can find only praise for Hill's accomplishment. It would be petty, indeed, to dwell upon its inevitable deficiencies.

WILLIAM L. STRAUS, JR.

ANIMAL MORPHOLOGY

THE CEPHALIC NERVOUS SYSTEM OF THE CENTIPEDE *Arenophilus bipuncticeps* (WOOD) (CHILOPODA, GEOPHILOMORPHA, GEOPHILIDAE). *Smithsonian Misc. Collection, Vol. 140, No. 4. Publ. 4425.*

By Michael A. Lorenzo. Smithsonian Institution, Washington. 75 cents (paper). iv + 43 pp. + 5 pl; text ill. 1960.

FACTS AND THEORIES CONCERNING THE INSECT HEAD. *Smithsonian Misc. Collections, Vol. 142, No. 1. Publ. 4427.*

By R. E. Snodgrass. Smithsonian Institution, Washington. 75 cents (paper). iv + 61 pp.; ill. 1960.

EVOLUTION OF CHORDATE STRUCTURE. *An Introduction to Comparative Anatomy.*

By Hobart M. Smith; illustrated by Alice Boatright. Holt, Rinehart & Winston, New York. $7.00. xiv + 529 pp.; ill. 1960.

Dedicated comparative anatomists are few and far between in these days of functional emphasis. It is, therefore, a pleasure to welcome a textbook authored by a knowledgeable expert and it is refreshing to read a new volume that is more than a rehash and rearrangement of existing texts. Smith not only is well informed in his subject but, in addition, reflects an abundance of practical teaching experience.

The text consists of 16 chapters devoted to the introduction, prevertebrates, classification, embryology, and the organ systems. As is nearly inevitable, the skeleton receives the lion's share of attention, with 3 chapters devoted to it. Curiously, there is an awkward chapter on endocrines of only 4 pages in length, including a few generalizations and 3 paragraphs on the adrenal gland. The remaining endocrines appear in other chapters, in accordance with their embryonic origin or tissue association. The typical chapter includes not only the descriptive material but ends with a number of questions for students, and a 3-column listing of new words—at times of formidable length (over 150 words in some tables).

Included in the volume is a good dictionary of Greek and Latin terms, without pronunciation; a good commentated set of references, both of a general nature and specific for each chapter; and the usual index. The illustrations are clear, adequate, and well-placed. The labels, however, are difficult for the reader with bifocals.

There are a few items about this book that will provoke discussion and reaction. Chief of these is the elaborate use of terminology. There is no relaxation from the use of scientific terms, and those who dislike the excessive use of a specialized jargon will excoriate this volume as an example of the wicked. In the pages on teeth, for example, the author introduces some 22 terms ending in "-dont," from acrodont to thecodont. It is, however, the coinage of new terms that particularly leaves one wondering whether all this is necessary for the presentation of comparative vertebrate anatomy. The author is aware of this propensity, and in his Introduction admits that his use of "protopostcava, protohepatic, protoposterior cardinal" may raise eyebrows. But he points out that "(1) a concept is more readily recognized if a name is available to it; (2) that it is confusing to lump different concepts under one name (for example, protopostcava and neopostcava); (3) that knowledge of the name for a concept is a quick and fairly accurate indication of the awareness of the concept itself, and (4) that learning the name for a concept is an aid to learning and recollection of the concept itself." Yet students often accept a name in place of understanding, and the poor zoological world is plagued with synonyms.

In the chapter on circulation, the treatment of the aortic arches is quite elaborate, reminiscent of that of E. S. Goodrich, and heavily loaded with the evolution of these structures in the fish groups. With all this detail, it is disappointing to find a cursory and probably incorrect treatment of the aortic arches in birds. Surely, the internal carotid is not a ventral aortic derivative in anybody's theory, and it is high time more attention was paid to what the situation in birds really appears to be.

It is a personal complaint of mine that comparative anatomists in general, including H. M. Smith, follow the unrewarding tradition of including much detailed early embryology. Why cleavage, gastrulation, and the concept of the cleidic egg have to take space in a vol-

ume of this kind, when feathers are left out, is beyond me. It is useful to consider the embryological aspects of evolution and to know of germ layers and their relations to organ systems, but that can be approached without a lengthy preamble on the nature of eggs. Here one would welcome a break with tradition.

These criticisms in no sense mean that this book is to be taken lightly. Indeed, it is one of the best in the field. It contains a wealth of coherent information with thoughtful evolutionary evaluations that will provide excellent grist for the mill at both student and teacher levels. There is no hesitation in recommending this scholarly work highly and in wishing the author and the book every success.

IRA B. HANSEN

THE ANATOMY OF *Callimico goeldii* (THOMAS). *A Primitive American Primate. Trans. Am. Phil. Soc., New Series, Vol. 49, Pt. 5.*

By W. C. Osman Hill. The American Philosophical Society, Philadelphia. $2.50 (paper). 116 pp.; ill. 1959.

Until recently, except for its skin and skeleton, the rare platyrrhine primate *Callimico* was quite unknown. Indeed, the only description of any of its soft parts prior to the present monograph is contained in a brief chapter, based largely on a single male specimen, in Hill's third volume on the order Primates (1957). This new and more extensive study, which includes additional material, is therefore a most welcome and important contribution to primatology.

Callimico has presented a taxonomic problem, for, whereas its extremities resemble those of the family Hapalidae, its dental formula resembles that of the family Cebidae. Hence some workers have regarded it as a hapalid, others as a cebid, and still others have placed it in a separate family, Callimiconidae. Basing his conclusions upon its total anatomy, Hill assigns *Callimico* to the Hapalidae, which includes the marmosets and tamarins. In fact, it appears to be the most primitive living member of that family. Hill's argument is convincing, at least to me. Moreover, his analysis of the characters of *Callimico* sheds considerable light on the much discussed problem of specialization within the Hapalidae. It is regrettable, however, that he did not make better use of the literature for comparative purposes.

WILLIAM L. STRAUS, JR.

ANIMAL PHYSIOLOGY

THE RETICULOENDOTHELIAL SYSTEM (RES). *Ann. N. Y. Acad. Sci., Vol. 88, Art. 1.*

By John H. Heller and Albert S. Gordon (Conference Co-Chairmen) and 36 other contributers. The New York Academy of Sciences, New York. $3.50 (paper). 280 pp.; ill. 1960.

RETICULOENDOTHELIAL STRUCTURE AND FUNCTON. *The International Society for Research on the Reticuloendothelial System. Third International Symposium, Rapallo, Italy, August 1958.*

Edited by John H. Heller. The Ronald Press Company, New York. $12.00. x + 473 pp.; ill. 1960.

Reticuloendothelial Structure and Function contains 32 papers presented at the Rapallo symposium. Although, in the words of the editor, John H. Heller, the volume "covers a wide range of subjects, including morphology, antibody synthesis, host defense mechanisms, properdin, steroids . . ." and "summarizes advances in a number of fertile areas . . . ," it cannot be considered a definitive work in its field. We have come to expect uneven quality in such collections of articles, but it is to be regretted that in this collection the preponderant number of contributions are neither clear, complete expositions of current research nor well-rounded summaries of earlier works. On the whole the volume is well illustrated; the book is attractive, easy to use, and well indexed. The price, however, represents a large investment for anything but a truly substantial reference book. Preliminary articles like A Comparative Evaluation of Properdin by Means of Inhibition of Hemolysis and Neutralization of the Phage, and The Effect of Zymosan on the RES belong in *Nature* or *Science*, and not in such works as this. Logical organization and synthesis are lacking; titles of references often are not given.

Much of the same ground is covered in *The Reticuloendothelial System (RES)*, a series of 23 papers resulting from a conference held at the New York Academy of Sciences in December, 1959. Eight or more authors (or groups of authors) took part in both conferences; fortunately, at least some of them have written different articles. Although most of the criticisms directed at *Reticuloendothelial Structure and Function* apply to *The RES*, the latter volume is an improvement in several respects. First, there is some element of organization of the papers; second, a few of the papers make an effort at synthesis; and finally, the price is only $3.50. The usual criticism of the New York Academy's *Annals* applies, however—the volume is not indexed. *The RES* is not a definitive work, either, but as an inexpensive indicator of the current status of research in the field, it should serve a useful purpose.

JAMES D. EBERT

L'ÉLECTROCARDIOGRAMME DYSMÉTABOLIQUE. *Perturbations Ioniques et Électrocardiogramme.*

By A. Larcan and C. Huriet; preface by P. Michon. Masson & Cie., Paris. 2,500 fr. (paper). viii + 221 pp.; ill. 1959.

This work surveys the literature on the effect of ionic imbalance on the electrocardiogram. The major part of the book is concerned with disturbances of blood potassium and the associated disturbances in the electrocardiogram which may be seen in a variety of diseases. The rest of the book is concerned with the effects of the imbalance in blood levels of other inorganic ions. An attempt is made to relate the somewhat complex electrocardiographic changes seen in clinically significant electrolyte imbalance to the recent studies of the ionic basis of the cardiac action potential. This book, which includes a bibliography of 511 entries, assembles a great deal of information on its subject and is therefore convenient as a work of reference in an area which is usually slighted in textbooks of electrocardiography.

PAUL CRANEFIELD

THE STRUCTURE AND FUNCTION OF MUSCLE. *Volume I: Structure.*

Edited by G. H. Bourne. Academic Press, New York and London. $14.00. xvi + 472 pp. + 23 pl.; text ill. 1960.

This is the first of a comprehensive 3-volume series to be devoted exclusively to muscle. The contributors to the first volume are the following: E. Andersson-Cedergren, E. B. Beckett, H. Stanley Bennett, G. H. Bourne, J. D. Boyd, Sybil Cooper, R. Conteaux, A. Csapo, Jean Hanson, H. E. Huxley, R. D. Lockhart, J. Lowy, M. R. Murray, F. D. Sjöstrand, and E. W. Walls. Starting with chapters on gross and microanatomy, the book proceeds through the development, histochemistry, ultrastructure, and molecular basis of contraction of striated muscle. There follow chapters on the structure and function of smooth muscle, invertebrate muscle, motor endplates, muscle spindles, and intercalated discs of heart muscles. The first chapter on the relation of the anatomy of muscles to movement and posture, written by R. D. Lockhart, is the only treatment of the classical concept of the function of muscle given in the entire series. While Lockhart's presentation is all right, as far as it goes, it is a pity that the editors could not have gotten Sir James Gray also to contribute a chapter on this subject. Particularly noteworthy are the chapters by H. Stanley Bennett on the electron microscopy of muscle, by H. E. Huxley and Jean Hanson on the molecular basis of contraction, and by Jean Hanson and J. Lowy on invertebrate muscle.

FRANCIS D. CARLSON

REPRODUCTION AND INFERTILITY. *III Symposium. Sponsored by The College of Veterinary Medicine and The Agricultural Experiment Station.*

Edited by F. X. Gassner; assoc. eds. Rue Jensen and H. J. Hill. Pergamon Press, New York, London, Paris, and Los Angeles. $6.50. x + 273 pp.; ill. 1959.

This volume includes papers of practical interest to the animal husbandman, particularly those contributions in Section I on Diseases of Reproduction in Male and Female, and in Section IV on Causes of Infertility. The papers on Ovarian Physiology (Sect. II) and on Steroid Metabolism and Physiology (Sect. III) are of a more basic nature. Among these may be mentioned: The Mechanism of Ovulation in the Cow; Maturation of the Sheep Ovum; The Relation of the Nervous System to Implantation, and Cytology of the Bovine Corpus Luteum. New data are presented on estrogen assays of blood and body fluids of cows and of pregnant urine in swine.

The reader of these proceedings is given two bonuses: (1) a not altogether pertinent introductory chapter on The Problem of Worldwide Radioactive Fallout from Nuclear Weapons Testing, by Langham of Los Alamos; and (2) round-table discussions on problems of infertility in dairy cattle and on problems involved in the use of frozen semen for artificial insemination.

CARL HARTMAN

TOXICOLOGY. *Mechanisms and Analytical Methods. Volume I.*

Edited by C. P. Stewart and A. Stolman. Academic Press, New York and London. $22.00. xviii + 774 pp.; ill. 1960.

With a team of 13 collaborators the editors have organized, in two volumes, the general methods of chemical analysis used in toxicology and a description of the principles on which these methods are based. Part I deals with the absorption, distribution, and excretion of poisons and their metabolites and includes much of pharmacologic interest. Part II (15 chapters) is concerned with methods of extraction, identification, and quantitation of specific classes of poisons.

The methods of analysis are given in detail, with discussion of the principles that will permit the toxicologist to modify the techniques for the identification of related new compounds. An extensive bibliographical author index has been included, in addition to the references cited at the end of each chapter. One is impressed by the modern nature of the analytical techniques described for toxicologic studies. Unfortunately, most of these methods are highly specialized and require great technical skill for their manipulation.

C. JELLEFF CARR

ANIMAL NUTRITION

PRINCIPLES OF NUTRITION.

By Eva D. Wilson, Katherine H. Fisher, and Mary E. Fuqua. John Wiley & Sons, New York; Chapman & Hall, London. $5.95. xii + 483 pp.; ill. 1959.

Intended primarily as a textbook "to provide basic information in nutrition at the elementary level, particularly for college students," this volume is organized and written with the presumption of negligible prior exposure to chemistry and physiology on the part of the reader. Considered as a whole, the volume is a commendable achievement. The development of the subject is orderly and logical. An early chapter is devoted to a brief introduction of scientific terms and concepts, although the selection of terms seems rather arbitrary. A glossary at the end of the volume supplements this chapter. The text is frequently documented with references to recent nutritional research literature, and many chapters are supplied with lists for suggested supplementary reading. Unfortunately, a large number of the references cited will be beyond the comprehension of the students at whom this volume is directed.

Commendation must be awarded for the readable method of presentation, for the interesting and subtle insertion of historical considerations, and for the considerable effort that has been made to simplify the discussion of chemical structure and chemical and physiological processes by the liberal use of schematic diagrams.

The volume is certainly not without its faults. A considerable amount of simplification is understandable in an undertaking of this nature, but scientific accuracy should not be sacrificed in the achievement of simplicity. The volume unfortunately is guilty of a number of inaccurate statements and definitions. Inconsistencies in definition, and even in spelling, are also to be found. It must be anticipated that these attributes will produce some confusion and misconceptions in the mind of the novice. Further confusion will be elicited by certain of the diagrams depicting chemical structures or processes. The frequent use of humanized atoms and molecules detracts considerably from the clarity of these diagrams.

Despite these criticisms, the book has considerable merit as a textbook for a formal course in elementary nutrition, a course wherein the protective hand of the instructor can guide the students around the sharp horns of any threatening dilemma.

K. J. MONTY

BIOPHYSICS AND GENERAL PHYSIOLOGY

PRINCIPLES OF GENERAL PHYSIOLOGY. *Volume 1: The Physico-Chemical Background.*

By L. E. Bayliss. Longmans, Green & Company, New York. $11.50. xiv + 520 pp.; ill. 1959.

PRINCIPLES OF GENERAL PHYSIOLOGY. *Volume 2: General Physiology.*

By L. E. Bayliss. Longmans, Green & Company, New York. $17.50. xxiv + 848 pp.; ill. 1960.

Today when almost all new textbooks in the biological sciences are produced by editors overseeing a group of authors each of whom writes a chapter or two, it is refreshing to find a textbook that has been written in its entirety by one man. In the Preface to Volume 2 the author discusses the scope of "general" physiology. He defines it as the study of the endowments of living material. General physiology considers problems common to all living things, whereas comparative physiology is in large part a description of functions peculiar to certain groups of organisms. General physiology is a synthesis of the disciplines of pharmacology, histology, biochemistry, and biophysics. This synthesis is not to be accomplished by any mere compilation of facts, but rather by emphasizing the general laws that govern the ways that living things function. This is the author's purpose. Now to the performance:

Volume 1 presents the principles of physics, physical chemistry, and biochemistry that are needed to understand the systems discussed in Volume 2. The main emphasis is upon electrochemistry, the properties of solutions and membranes, and the colloidal state. This part is more in the nature of a refresher or review. The material is handled in a simple, straightforward manner, and several of the sections, in particular the ones on membranes, are excellent.

Volume 2 considers problems of life under the following headings: The living cell and its environment; Nutrition; Excitation and inhibition; Effector organs; Receptor organs; Integrating systems, hormones and nervous systems; Transport of metabolites and the circulation of the blood; Growth and reproduction; and Homeostasis and control systems.

The author maintains a general approach to life wherever possible, although he recognizes that general laws cannot be adequately described and explained without concrete instances. It is here that the chief fault of the book lies. Physiology has now reached a stage where we are overwhelmed with a mass of facts about all types of animals. We find that closely related animals may have large differences in structure and function. A general statement about

the organization of visual systems may in fact describe only a few animals adequately. The examples are chosen quite at random, although a majority are mammalian, and usually human. The reader is often not quite sure whether a particular animal is being discussed or just some general organism which has characteristics of all living beings simultaneously. On the positive side, however, the work has many merits. The single authorship of the book has insured a uniform style which is quite readable and generally lucid. The material is up to date and generally well selected. Redundancy has been held to a minimum. When a mathematical treatment of the subject has been used, much attention is devoted to clarifying the physical basis for all the variables and constants in the equations. The bibliography is adequate without being overwhelming, although it would be helpful if the papers somewhat obscurely cited in the legends of figures were also included in complete form in the bibliography. The author has made a good attempt to write a "general" physiology textbook. His lack of success is due more to the confusing aspect of modern physiology than to anything else. Modern physiology should be compared to modern physics with its multiplicity of fundamental particles. At the present moment we have more facts than our theories can handle; perhaps we should concentrate on comparative physiology, out of which in time a truly general physiology will arise.

MYRON L. WOLBARSHT

INTRODUCTION TO SUBMOLECULAR BIOLOGY.

By Albert Szent-Györgyi. Academic Press, New York. $5.00. x + 135 pp.; ill. 1960.

This small monograph, in addition to being engaging reading, provides a boon for the general biologist, as well as for many physical scientists. Szent-Györgyi directs attention to the basic problems of energy transfer in biological systems, or "bio-energetics," and points out that the ultimate explanation of such phenomena must be reduced to the electronic or submolecular level. He proceeds, thereupon, to explain some of the concepts of quantum mechanics in non-mathematical terms readily understood by the uninitiated and to show qualitatively how these ideas can be used to interpret a variety of interactions between biologically interesting molecules, particularly those involving charge-transfer. As the scientific world is now accustomed to find, he continues thereafter to describe a variety of his own experiments designed along unconventional lines and leading to very striking and unprecedented observations. Finally, he concludes with conjectures along submolecular lines on some broad biological problems including the mechanism of drug action and the nature of the living state.

This volume is permeated with the personality and wit of the author, and offers all biologists an afternoon of stimulating and pleasant reading. As in the past, Szent-Györgyi's intuition and enthusiasm will also prompt many theoretical and experimental scientists to devote attention to some of the problems he has penetrated, even though they have previously been considered inaccessible.

I. M. KLOTZ

PHOTOPERIODISM AND RELATED PHENOMENA IN PLANTS AND ANIMALS. *Proceedings of a Conference on Photoperiodism, Oct. 29–Nov. 2, 1957, sponsored by the Committee on Photobiology of the National Academy of Sciences—National Research Council and supported by the National Science Foundation. Publ. No. 55.*

Edited by Robert B. Withrow. American Association for the Advancement of Science, Washington. $14.75; $12.50 (AAAS members). xviii + 903 pp.; ill. 1959.

The proceedings of this conference, held in Gatlinburg, Tennessee, in 1957, have been published as a single large volume. The book is bulky (903 pages) and expensive ($14.75) and will be of only limited use to any individual investigator. Periodic phenomena have by now been discovered in almost all types of organisms, and at every level from the rates of biochemical reactions to the gross behavior of advanced animal species, including man. The experimental data reported in the present volume range throughout this gamut of fields of investigation. It will be extremely rare that any single person will find an equal interest in or comprehension of even a majority of the phenomena described in the 57 contributions to the volume. On the other hand, if a review is desired on periodic activities in the organisms of one's choice, this book contains articles on higher plants, chickens, a dinoflagellate, reptiles, midges, mice, men, crustaceans, bobolinks and assorted other birds, insects, and mammals.

Attempts are made with varying degrees of intensity to provide a theoretical and general framework for describing the mechanisms of periodic phenomena. Noteworthy among these are the articles by C. S. Pittendrigh and V. G. Bruce, and by E. Bünning. As of the date of publication of the volume, the mechanisms suggested were all quite abstract. In spite of the bulk of this volume it remains evident that the basic mechanisms creating the rhythmic phenomena are entirely unknown, in all cases. The conference represented a tribute to a major challenge in modern biology much more than a record of its conquest.

Roughly one half of the book (and 38 out of the 57 articles) is devoted to higher plants. This section is

seriously marred by the inclusion of a large number of trivial papers, reporting preliminary data often presented at greater length in the original literature later. These had only a transitory value for the specialist, and none whatsoever to the non-specialist looking for a broad view of the field. The blame for the inclusion of many of them may be laid to the editorial policy (announced in the Preface) of encouraging the submission of short articles as a substitute for recording the extemporaneous discussion. As a tentative guide for a reader looking for articles of larger interest in the plant section, at least those by the late R. Withrow, by C. S. French et al., S. Hendricks, H. F. Wareing, W. S. Hillman, J. P. and Colette Nitsch, I. Spear, A. Lang, F. B. Salisbury, H. A. Borthwick, and R. H. Wetmore et al. are of considerable value and more fully developed.

A. JAGENDORF

BIOCHEMISTRY

MECHANISMS OF INORGANIC REACTIONS. *A Study of Metal Complexes in Solution.*

By Fred Basolo and Ralph G. Pearson. John Wiley & Sons, New York; Chapman & Hall, London. $11.75. xiv + 426 pp.; ill. 1958.

This text is a thorough exposition of modern ideas in inorganic chemistry. A listing of the chapter titles indicates the scope of the work: The Theory of the Coordinate Bond; Substitution Reactions of Octahedral Complexes; Substitution Reactions of Square Complexes; The Stereochemistry of Substitution Reactions of Octahedral Complexes; Isomerization and Racemization Reactions; Oxidation-Reduction Reactions; and Catalytic Effects of Coordination Compounds. The descriptive material, a theoretical treatment of inorganic complexes, and an outline of the mechanisms of inorganic reactions are treated in some detail. *Mechanisms of Inorganic Reactions* is recommended, in particular, to those who wish a fuller understanding of the role of metal ions in biochemical systems. The chapters on oxidation-reduction and catalysis are especially good.

EMIL H. WHITE

BIOCHEMICAL PREPARATIONS. *Volume 6.*

Editor-in-Chief: Carl S. Vestling. John Wiley & Sons, New York. $5.25. x + 105 pp.; ill. 1958.

BIOCHEMICAL PREPARATIONS. *Volume 7.*

Editor-in-Chief: Henry A. Lardy. John Wiley & Sons, New York and London. $5.25. x + 102 pp.; ill. 1960.

In Volume 6, explicit directions are given for the preparation crystalline animal cytochrome *c*, deoxyribonucleic acid, 2,3-diphosphoglyceric acid, L-α-glycerophosphorylcholine, 3-hydroxyanthranilic acid, β-hydroxy-β-methylglutaric acid, insulin, lanosterol from "isocholesterol," leucine aminopeptidase, α-methylserine and bis (hydroxymethyl)glycine, crystalline horse oxyhemoglobin, old yellow enzyme, crystalline papain and benzoyl-L-argininamide, crystalline muscle phosphorylase *b* and phosphorylase *a*, crystalline muscle phosphorylase *a*, phosphoserine, ribonucleic acid from rat liver, ribulose diphosphate, and DL-tryptophan-7α-C^{14}.

In Volume 7, directions are given for the preparation of N-acetylneuraminic acid, adenosine di- and triphosphates containing P^{32} (both enzymatic and chemical methods are given), β-aminoisobutyric acid, carbamyl phosphate, (−)-carnitine chloride, cerebrosides, coproporphyrin III tetramethyl ester, 6-deoxy-6-fluoro-D-glucose, dihydroxyacetone phosphate, flavin adenine dinucleotide (FAD), D-fructose-1-phosphate (barium salt), 3-hydroxy-L-kynurenine, hydroxypyruvic acid phosphate, methylmalonic semialdehyde, potassium dihydrogen L_{cr}-(+)-isocitrate, S-palmityl coenzyme A, stearic acid, methyl stearate and homologous compounds, stigmasterol, and tetrahydrofolic acid (5,6,7,8-tetrahydropteroylglutamic acid).

For each preparation, assay and purification methods are given and also a discussion of other methods for the synthesis. Each volume, in addition, contains a list of compounds of biochemical interest that have appeared in *Organic Syntheses.*

EMIL H. WHITE

ORGANIC PEROXIDES IN RADIOBIOLOGY.

By R. Latarjet and 21 other contributors. Pergamon Press, London, New York, and Los Angeles. $9.50. viii + 153 pp.; ill. 1958.

This book contains 16 articles written by specialists in radiation chemistry and various areas of radiobiology. There is a 10-page general discussion at the end of the book, in addition to specialized discussions at the ends of the papers. In current radiobiological thinking a prominent role is played by "intermediates," i.e., chemical species which are known to be formed when suitable aqueous solutions are exposed to high-energy radiation and whose formation therefore can reasonably be expected in irradiated cells. Prominent among these intermediates are certain free radicals (H, OH, HO_2), hydrogen peroxide, and organic peroxides. The colloquium reported here was designed to consider especially the organic peroxides. Several of the papers deal with the production of such peroxides in irradiated solutions containing organic molecular species known to exist in cells (e.g., lipids, nucleic acids and related

compounds, amino acids, and peptides). Several others describe injurious effects of certain organic peroxides not produced by irradiation and of irradiated products suspected of containing such peroxides. Most of the remaining contributions are concerned with molecular oxygen as a modifier of the radiosensitivity of cells and cell components, with special reference to the possibility that this widely observed "oxygen effect" may operate through use of the O_2 in producing organic peroxides, which then in turn may be involved in the actual damage observed.

Not surprisingly, the participants came to no general conclusion concerning the possible role of organic peroxides in radiobiology, but apparently, to judge from the spirited discussion, there was a great measure of mutual stimulation. This stimulation is transmitted to the reader and probably is the most valuable feature of the book.

The colloquium included 36 participants, chiefly from laboratories in France and England. Half of the papers are in English and the remainder in French. The discussions are also bilingual, sometimes even within the same sentence. Both the papers and the discussion are quite readable. The book certainly is not for beginners, but should be familiar to all who are concerned with the molecular mechanisms by which radiations produce their chemical and biological effects.

R. E. Zirkle

Enzymes. *Lectures held at the Conference on Enzymes and their Action, Wageningen, April 6–9, 1959.*

Introduction by E. C. Slater. N. V. Uitgevers-Maatschappij W. E. J. Tjeenk Willink, Zwolle. 158 pp.; ill. 1959.

This book is the second of a series of conferences on fundamental subjects in the field of the exact sciences. The purpose of these colloquia is to provide information to research workers in the physical sciences about recent developments in areas of study foreign to their own.

Each chapter records a lecture given at the conference. Each lecture is presented by an established expert on the specific aspect being considered. Taken cursorily, each lecture contributes merely to an overall, coherent view of the broader aspects of enzymology. Upon more careful perusal, however, a much more definite composition is unveiled in which there is an interplay of the sometimes drab and cold summary of fundamental principles with the more interesting suggestion of questions unanswered and problems yet untouched.

After an introductory sketch by E. C. Slater, the mosaic field of modern enzymology is methodically pieced in succeeding lectures: The chapters by C. DeDuve and P. Desnuelle describe the present knowledge of the intracellular localization and structure of enzymes, and the techniques used in studying these aspects. E. C. Webb and Rudolph A. Peters review basic principles relating to coenzymes and inhibitors of enzymes, respectively. Some of the major questions challenging the enzymologist today are pointed up in the lecture by F. Dickens on the role played by enzymes in the regulation of metabolism. Finally, the problems involved in the biosynthesis of enzymes and of proteins in general are discussed by H. Chantrenne.

Considering the size of the task attempted in this tiny volume, the result is admirable. That 7 chapters could cover so remarkably well such a fast-moving, ever-widening interdisciplinary field is a reflection of the careful selection of topics by E. C. Slater, the organizer of the conference, and the scholarly treatment afforded each topic by the participants.

For the research worker outside of enzymology this book will serve its purpose well. Nor does its worth end there, for it may also serve as an introductory outline and summary for the student, as a nucleus about which a teacher could construct a lecture course in basic enzymology, or even as a refreshing composite for the specialist in biochemistry.

C. DeLuca

Ciba Foundation Symposium on the Regulation of Cell Metabolism.

Edited by G. E. W. Wolstenholme and Cecilia M. O'Connor. Little, Brown & Company, Boston. $9.50. xii + 387 pp.; ill.

The genotype of a cell in part controls the phenotype by containing the hereditary potential for the synthesis of a vast array of polypeptide chains comprising probably an equally vast aray of enzymes. The symposium omits any discussion of this problem and commences a step further on. Cells also have the capacity to synthesize only a particular portion of the potential enzyme complement, that is, to determine the amount of each enzyme which will be made at a specific time. This attribute is influenced by the heredity of the cell, by the prior history of the cell or one from which it was derived, and by the environment. B. Magasanik, A. K. Magasanik, and F. C. Neidhardt admirably review their own experiments bearing on this question. The regulation of cell metabolism by the composition of the enzyme complement is reflected in several other articles. Reaction rates in one pathway may be influenced by the availability of substrates or regeneration of cofactors, determined in part by limiting reactions in otherwise unassociated metabolic pathways. For example, H. Holzer points out that limitations in glucose oxidation may be brought about by inade-

quate reoxidation of essential coenzymes. Built into (some?) enzymes is another important aspect of control independent of the precise enzyme complement the cell contains. A. B. Pardee points out that certain key enzymes, initiating particular metabolic pathways, are extremely sensitive to "feedback inhibition" of enzyme activity by the ultimate product of the reaction sequence. In addition, the cell is not a bag of enzymes; it is compartmented; and apparently built into enzymes is an affinity for a particular position in the cell or in one of the cell's substructures. Several papers in the symposium discuss the compartmentalization of the cell and the importance of this subdivision in the regulation of metabolism. For example, intramitochondrial ADP content controls relative rates of respiration and oxidative phosphorylation while extramitochondrial ATP concentration controls the rate of fermentation. Current discussion centering on the control of these last processes is the major concern of the symposium, which was arranged by Sir Hans Krebs. While the symposium reviews developments in an area critical to an understanding of the chemical basis of growth and development, its primary interest will be to those especially concerned with fermentation and oxidative respiration and their interrelationships in metabolic control.

PHILIP E. HARTMAN

VITAMIN B_{12}. *Methuen's Monographs on Biochemical Subjects.*

By E. Lester Smith. John Wiley & Sons, New York; Methuen & Co., London. $3.00. xii + 196 pp.; ill. 1960.

This volume is a real credit to the familiar series of Methuen Monographs. A very large amount of highly diversified information is presented to the reader in a concise, critical, and easily readable form. The organization of subject matter is extremely good, sparing the reader the necessity of flipping pages between cross-references. The author states that the monograph was intended to be a comprehensive, but not all-inclusive, review of the pertinent literature. This goal seems to have been achieved very satisfactorily.

The contents include a brief historical discussion of the experimental endeavors leading to the discovery of vitamin B_{12}, a consideration of the distribution and isolation of the vitamin, a detailed summary of the efforts which elucidated its chemical structure, a brief review of the evidence concerning its biogenesis, a discussion of nomenclature, 3 chapters concerned with naturally occurring and synthetic modifications of the parent vitamin and with the biological significance of these, a review of pertinent analytical methods, a consideration of "intrinsic factor," a discussion of physiological problems involving vitamin B_{12} in mammals, including absorption, excretion, nutrition, and megaloblastic anemia, and finally a chapter concerned with possible roles of the vitamin in intermediary metabolism.

The monograph may be considered as a highly informative review of the literature through mid-1958. It consequently was completed too early to include a discussion of the nature and role of the coenzymes derived from vitamin B_{12} and its analogues. Despite this omission, the monograph will be a real asset to any biochemical library and a pleasurable and profitable reading experience for most biologists.

K. J. MONTY

LIPIDE METABOLISM.

Edited by Konrad Bloch. John Wiley & Sons, New York and London. $10.50. xiv + 411 pp. 1960.

This volume is a continuation of the recent book by D. J. Hanahan entitled *Lipide Chemistry* (*Q.R.B.*, 35:163. 1960). In the present volume, as in the previous one, each chapter is written by a specialist in the field. The first chapter, on the enzymatic mechanisms of fatty acid oxidation and synthesis, is written by David E. Green and Salih J. Wakil. It summarizes the recent advances in fatty acid oxidation by mitochondrial systems of animal tissues as well as the oxidation of fatty acids in plants and microorganisms. Similarly, the synthesis of fatty acids in a variety of systems is considered.

The metabolism and interconversion of unsaturated fatty acids is discussed adequately by J. F. Mead. This investigator discusses his own work as well as that of others in the field of the biosynthesis of lipids in intact animals as well as current concepts concerning the hydrogenation, degradation, and interconversions of various fatty acids.

The metabolism and function of phosphatides is contributed by R. J. Rossiter and K. P. Strickland. In this section different schemes for the formation of lecithin and other phosphatides are presented. The role of phosphatides as structural components of cells, agents in protein synthesis, electron carriers, and intermediaries in secretory activity are discussed. Other aspects, such as their role in the absorption and transport of fat and blood coagulation, are presented in a well-organized fashion.

B. Borgström, from the University of Lund, Sweden, discusses next the digestion and absorption of glycerides, followed by a discussion of the distribution and metabolism of these lipids. This chapter presents the factors regulating the plasma lipid concentrations.

Newer knowledge on lipolytic enzymes is reviewed by Morris Kates, of the National Research Council

of Canada. The lipases, phosphatidases, lysophosphatidases, and other enzymes involved in the hydrolysis of phosphatides are presented sequentially, and the use of enzymes for determining the structure and configuration of phosphatides is discussed. Work on other lipolytic enzymes, such as cerebrosidase, cholesterol esterase, and vitamin A esterase, also is briefly reviewed.

The hormonal regulation of fatty acid metabolism is presented by R. G. Langdon, who stresses particularly the roles of insulin, epinephrine, and anterior pituitary hormones in fatty acid metabolism and the integration of the metabolism of carbohydrates and lipids.

The formation and metabolism of bile acids is discussed by S. Bergström and collaborators from the Karolinska Institutet, Sweden. In this chapter the transformation of bile acids by microorganisms and the metabolism of bile acids in different mammalian species are presented. Isotope studies on the quantitative aspects of bile acid metabolism in rat and man, primarily derived from the author's own laboratory, are also given in detail.

Finally, the chemistry and metabolism of bacterial lipids are discussed by F. Asselineau and E. Lederer (Paris), who review the chemical composition of bacterial lipids as well as their metabolism and biological activities.

The present volume covers a wide area of research, although it does not claim to be all-inclusive. It would appear that for the general reader as well as for the specialist in the area of lipid metabolism a great deal of interesting material has been assembled by the editor.

D. B. ZILVERSMIT

HISTOCHEMISTRY. *Theoretical and Applied. Second Edition. By A. G. Everson Pearse. Little, Brown & Company, Boston.* $20.00. x + 998 pp. + 10 pl., text ill. 1960.

The first edition of this book (*Q.R.B.*, 29:187. 1954) has proved to be one of the most comprehensive and useful books on the subject ever produced. The second edition continues in this tradition. The section dealing with structural proteins is particularly well done, and the number of chapters dealing with enzyme methods has been enlarged. In a field moving as rapidly as protein histochemistry, it is difficult to keep up to date in a major treatise of this sort, and it is certain that additional editions will be required. While there may be some dispute among cytochemists concerning some of the procedures and proposed reaction mechanisms presented, the material in this book is sound and represents the best general consideration of histochemical methodology now available. The practice of including the details of techniques in appendices at the end of the book has been retained in the present edition and adds considerably to the usefulness of the book.

R. R. COWDEN

MICROBIOLOGY

BIOLOGY OF THE PLEUROPNEUMONIALIKE ORGANISMS. *Ann. N. Y. Acad. Sci., Vol. 79, Art. 10. By D. G. ff. Edward and 79 other contributors. The New York Academy of Sciences, New York.* $5.00 (paper). iv + pp. 309–758; ill. 1960.

Most biologists are aware of man's rapidly expanding view of the microbial world, attributable to the discovery, for example, of a striking new class of organisms represented by the *kappa* particles of *Paramecium* or of entire groups of pathogenic viruses. However, few are aware of the existence of another large group of organisms, the pleuropneumonia group, which are closely related to bacteria. The morphology, immunological properties, growth in vivo and in vitro, and taxonomic postion of these organisms have received concerted attention only in the last few years. Their involvement in the pathological conditions of human beings and of animals economically important to man and their widespread occurrence in tissue cultures are now becoming apparent for the first time. The current symposium comprises over 50 papers dealing with various aspects of these fascinating microorganisms. Its publication symbolizes a transition now taking place in their study: persons newly interested join the pioneers in this area of investigation, most notably H. E. Morton, L. Dienes, D. G. ff. Edward, and E. Klieneberger-Nobel, their students, and associates.

PHILIP E. HARTMAN

AN INTRODUCTION TO THE BIOLOGY OF MICRO-ORGANISMS. *By Lilian E. Hawker, A. H. Linton, B. F. Folkes, and M. J. Carlile. Edward Arnold, London.* 35s. viii + 452 pp. + 40 pl.; text ill. 1960.

Few introductory textbooks can even approach this very fine new volume in balance, organization, and coverage of the microbial world. The volume is profusely illustrated, neatly set, and reasonably priced; it contains a superior index. The writing accurately reflects current scientific thought and is always clear and readable. This may be due partially to a disregard for detail. All scientific jargon, terminology, and detailed references but those which are essential are omitted from the text in favor of a few

more critical general sources recommended for further reading and placed at the end of each chapter. More likely, however, the clarity is a reflection of the authors' familiarity with the diverse groups of organisms they describe and a fortunate facility in expression. While the fact that the text covers no single subject thoroughly might be decried, the contents are still enormous. A great deal of the microbial world is described, largely in comparative fashion, in this excellent general introduction to the morphology, life cycles, physiology, metabolism, and ecology of microorganisms.

PHILIP E. HARTMAN

HEALTH AND DISEASE

THE INFLUENCE OF HORMONES ON LIPID METABOLISM IN RELATION TO ARTERIOSCLEROSIS. *Ann. N. Y. Acad. Sci., Vol. 72, Art. 14.*

By Abraham Dury and Carleton R. Treadwell and 35 other contributors. New York Academy of Sciences, New York. $4.00 (paper). ii + pp. 789–1053; ill. 1959.

This monograph is an important companion to several others, published recently, which deal with the same general subject. The group of them provides a comprehensive source of information upon many viewpoints of the atherosclerosis problem at the basic level.

The present volume consists of a series of papers by many authors of established reputation. There are 4 parts, the first of which, consisting of 5 papers, deals with such general subjects as the biosynthesis of lipids, cholesterol absorption, lipoprotein structure, and finally of chylomicron and lipid transport. The papers are skilfully designed to provide a fundamental chemical background for the subsequent consideration of metabolic and hormonal control of basic processes at the tissue level.

The second and third sections contain 8 papers. These are concerned with the integration of lipids and pituitary, adrenal, gonadal, thyroid, and pancreatic hormones in the atherosclerosis process. For the most part, the papers of this section are relevant, even when the available facts are meagre. However, several wander too far from what seems sufficiently related to vascular disease as it occurs in man to be given such detailed consideration.

The final section deals with the arterial wall, its ground-substance, enzyme activities, and hormone influences upon it. Though this section deals with what has been least investigated, the fundamental importance of the arterial wall and its dynamic behavior in atherosclerosis makes it essential to continue and expand investigation along lines that are presented here.

Two papers in particular merit mention because of their appeal to readers with clinical interests. The freedom from atherosclerosis usually enjoyed by premenopausal women is convincingly presented. "Favorable" effects of estrogens upon lipid patterns are contrasted with those thought to be "unfavorable" and which result from the action of androgens. Practical limitations to the use of estrogen therapy are made clear. In connection with therapy the crucial question—so crucial that it cannot be stressed too often—is raised and the lack of any answer to it is emphasized. No matter how tempting the circumstantial evidence may be, it has not yet been demonstrated in any way convincingly that alterations of blood lipids by any means alters the expectancy of myocardial infarction in normal populations.

The paper dealing with the thyroid points out again that atherosclerosis is common in patients with myxedema and is uncommon in those with hyperthyroidism. Because of high blood lipid levels in the former and low levels in the latter, it is pointed out that the data are commonly construed as supporting the high fat diet theory of atherogenesis. The data are critically examined, and the conclusion is drawn that the apparent frequency of atherosclerosis in some patients with underactive thyroids may not be due to elevated blood lipid levels.

The monograph will not appeal to those who might expect from popular expressions of loose thinking on the subject to find that the mystery of atherosclerosis is about to be dispelled. The serious student knows better, that the mystery remains essentially unsolved and that its solution will come almost certainly only from investigations at a very basic level. Such students will find in this monograph guidance and a rich source of ideas.

BENJAMIN M. BAKER

SOIL, GRASS AND CANCER. *Health of animals and men is linked to the mineral balance of the soil.*

By André Voisin; translated by Catherine T. M. Herriot and Henry Kennedy. Philosophical Library, New York. $15.00. xviii + 302 pp.; ill. 1959.

This reviewer is frank to admit that an a priori consideration of the volume *Soil, Grass and Cancer*, created the impression that its author was probably one of the too numerous faddists or pseudo-scientists who operate on the fringe of existing knowledge and that its contents would be a confused potpourri of fact and prejudice. Nothing could have been further from the truth!

Voisin, basically a farmer, is a member of the French Agricultural Academy. His text is concerned with the mineral balance of the soil, its effect on fod-

der, and eventually the health of animals and man. It reviews the known relationships between alteration of the elements of the soil and such disorders as tetany, ataxia, anemia, diarrhea, goiter, and others. Furthermore, it contains many speculations about the importance of soil composition in the pathogenesis of numerous human ills, among them cancer.

Many of Voisin's contentions are unproven and may be disputed. Nonetheless, his book focuses the informed lay and scientific readers' thoughts on the broader aspects of nutrition, food production, and an aspect of preventive human medicine which is too often neglected. The English translation is quite good and the text may be read easily and rapidly.

ALBERT H. OWENS, JR.

PATHOGENESIS AND IMMUNOLOGY OF TUMOURS. *Edited by G. V. Vygodchikov; translated from the Russian by R. Crawford. Pergamon Press, New York, London, Paris, and Los Angeles.* $12.50. viii + 258 pp.; ill. 1959.

In a concise volume, the editor has compiled an authoritative collection of papers by leading Soviet specialists in the field of tumor epidemiology and immunology. The subject matter is discussed under four main headings: Pathogenesis of Malignant Tumors; Antigens, Immunity and Antibodies; and Vaccination. There is also a brief report on electron microscope study at the end of the book. The index is excellently cross-indexed as to subject matter.

Many of the articles report work that is similar to researches now being carried out in America, England, and Europe. However, the importance and interest of the Russian investigations into the etiology and immunology of tumors cannot be underestimated. It is in these fields that the Soviet doctors take the lead in their basic and original research.

ROBERT G. CHAMBERS

HODGKIN'S DISEASE. *Ann. N. Y. Acad. Sci., Vol. 73, Art. 1. By Antonio Rottino and 33 other contributors. The New York Academy of Sciences, New York.* $4.50 (paper). 379 pp.; ill. 1958.

The series of papers contained in this volume is the result of a conference held in November, 1957. The title of the conference is to some extent deceptive. Although interest in this relatively rare and apparently exclusively human neoplastic disease has always been great, fundamental advances toward a more complete understanding of its nature have been few, a fact that would suggest that these collected presentations might contain little of current interest.

An appreciation of the topics and contributing authors, however, should arouse the interests of individuals of several scientific persuasions. The lymphocyte, the eosinophil, and the reticuloendothelial systems are considered from the viewpoints of the morphologist, the biochemist, and the biologist. One purpose of the conference was to emphasize the possible pertinence of these more recent studies to the problems of Hodgkin's disease, and in this it succeeds quite well. A final section devoted to a review of the therapeutic management of Hodgkin's disease contains little new information and no data that are not available from other sources.

ALBERT H. OWENS, JR.

SCREENING PROCEDURES FOR EXPERIMENTAL CANCER CHEMOTHERAPY. *Ann. N. Y. Acad. Sci., Vol. 76, Art. 3. By Chester C. Stock and 78 contributors. The New York Academy of Sciences, New York.* $5.00 (paper). iv + pp. 411–970; ill. 1958.

This monograph was sponsored conjointly by the Cancer Chemotherapy National Service Center and the New York Academy of Sciences. The major purpose of the conference on which the monograph was based was to provide the Service Center with a review of the state of knowledge in the field of anticancer screening.

The monograph consists of 52 excellent papers discussed under separate headings: Microbiological Studies; Normal and Malignant Cells and Tissue Culture; Ascites Tumors; Lymphoma; Hormone Responsive Tumors; The Human Tumor in Heterologous Hosts; Special Systems; and Consideration in Experimental Cancer Chemotherapy. As stated in the excellent and concise summary prepared by Chester C. Stock, this publication was designed as an examination of new test objects and new procedures and as an evaluation of old techniques. In my opinion, the monograph adequately fulfills its purpose. The monograph contains a 6-page Appendix which lists and summarizes the results of anti-tumor tests on 200 selected compounds.

ROBERT G. CHAMBERS

PSYCHOPHARMACOLOGY FRONTIERS. *Proceedings of the Psychopharmacology Symposium of the Second International Congress of Psychiatry held in Zürich, Switzerland, on September 2–4, 1957. Edited by Nathan S. Kline. Little, Brown & Company, Boston and Toronto.* $10.00. xxiv + 533 pp.; ill. 1959.

The contents of this symposium are, for the most part, either out of date, irrelevant, or already well-known. There seems little reason for anyone to purchase this book, except perhaps the participants who might treasure a souvenir of the occasion.

LOUIS LASAGNA

PHARMACOLOGY AND THERAPEUTICS. *A Textbook for Students and Practitioners of Medicine and its Allied Professions. Fourth Edition.*

By Arthur Grollman. Lea & Febiger, Philadelphia. $12.50. 1079 pp.; ill. 1960.

This popular textbook has been brought abreast of the recent developments in pharmacology; indeed only two years have elapsed since the appearance of the last edition, so rapid is the pace. Grollman has reiterated his goal of continuing the "severely critical and rigorously scientific" approach to the subject which characterized the original text of Arthur Cushny. To the subject matter, the author has brought his own engaging style of writing, which is best displayed perhaps, in Chapter 20, Drugs Acting Primarily on the Vascular System.

The text is divided into 9 parts that follow the classical subdivisions of pharmacology. There is an appendix on prescription writing, and a therapeutic use appendix which serves as a therapeutic index to the text descriptions of the pharmacology of these drugs. The book is a good textbook for students of medicine.

The major issue that writers of pharmacology textbooks must resolve is how to present to the student the bewildering number of drugs with their duplicate names, in some orderly form. Rigid discipline and a native sense of the pharmacologically important must be exercised lest the sheer weight of the volume become overwhelming. Grollman has succeeded quite well in this regard. However, after pointing out the unimportant aspects of materia medica and pharmacognosy, why devote valuable space to rather poor drawings of botanical drug plants? Actual photographs would have been superior if this type of construction is really necessary for medical students. Again, one is puzzled to find photographs of vitamin deficiencies in animals when most high school students today are conversant with these aspects of nutrition.

The lack of paragraph headings makes for difficult reading when one is trying to find specific information about drug actions. Certainly, the Latin names for official preparations may be deleted. The English names for drugs will be confusing to students in the United States, e.g., Leptazol for pentylenetetrazol. And why retain an outmoded (1926) name like Cardiazol? Unfortunately, only U.S.P. and B.P. drugs and their preparations are described in the section at the end of each chapter that gives the physical properties and doses. The very numerous unofficial drugs are omitted in this respect. Not any of these matters should be considered a serious deficiency of the work, and undoubtedly many classes of students will find the book a valuable tool to broaden their pharmacologic horizon.

C. JELLEFF CARR

HUMAN BIOLOGY

PHYSICS AND MEDICINE OF THE ATMOSPHERE AND SPACE. *The Proceedings of the Second International Symposium on the Physics and Medicine of the Atmosphere and Space held at San Antonio, Texas, November 10, 11, 12, 1958.*

Edited by Otis O. Benson, Jr. and Hubertus Strughold. John Wiley & Sons, New York and London. $12.50. xviii + 645 pp.; ill. 1960.

A more descriptive title for this publication would be "Physical and Biological Considerations for Space Travel," since the text is definitely oriented toward the hazards and difficulties of putting man into space. There are 42 separate papers, each a fairly specialized treatment of some aspect of this problem. Approximately half of the papers relate to such physical considerations as the environment of space, types of propulsion systems, space vehicle engineering, etc., and the remaining papers discuss the biological aspects of space travel pertaining to human adaptation, nutrition, perceptual and sensory difficulties, escape, survival, and the like. Many of the contributing authors are quite well known and respected as authorities. All of the articles are well written in a scholarly manner. It is obvious that much of the material reflects fresh data and new concepts. The following brief selection of names and titles will illustrate the depth and breadth of the coverage: On the Radiation Hazards of Space Flight (James A. Van Allen); The Timing of Satellite and Space Probe Launchings (Wernher von Braun); Manned Orbital and Lunar Space Vehicles (Krafft A. Ehricke); A Survey of Possible Propulsion Systems (L. R. Shepherd); Biomedical Aspects of Orbital Flight (W. Randolph Lovelace II and A. Scott Crossfield); Human Tolerance to Accelerations of Space Flight (John Paul Stapp); and Survival in Space (Alfred M. Mayo). This publication will enlighten any reader as to the current state of the art among the scientists and engineers who are attempting to solve the difficulties of space travel.

H. G. WAGNER

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